SCIENCE

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ADDRESS BY THE PRESIDENT OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.[1]

II.

AT Cape Town I attempted to make a general survey of evolution in its various branches, and laid down certain general propositions as to what seems common to all of them.

I then went on to consider how these general laws found an application in the most recent speculations as to the constitution of matter. The atoms of the elements and the molecules of chemical combinations are constructed on so minute a scale that it is no easy task to picture them to our minds. On the other hand, we see in the heavens arrangements of matter on a scale so vast that it is equally difficult to grasp. Both the inconceivably small and the inconceivably large should fall under a general law, if it is a true one; and the history of satellites, planets and stars presents at least as great an interest as that of atoms and molecules. Accordingly the transition from the small to the large seemed to afford a convenient halting place in my address, and I propose to-night to resume the discussion by considering various theories of celestial evolution. But I will first try to render the point of view intelligible which I desire to take. A short preliminary explanation for those who were not at Cape Town thus becomes necessary.

I desire to present the essential features which are common to evolution in all its branches, and this may be done most easily

[1] Johannesburg, South Africa, August 30, 1905.

by reference to political institutions, because we all fancy we understand something of politics.

The complex interactions of man with man in a community are usually described by such comprehensive terms as 'the state,' 'the commonwealth' or 'the government.' Various states differ widely in their constitution and in the degree of the complexity of their organization, and we classify them by various general terms, such as 'autocracy,' 'aristocracy' or 'democracy,' which express somewhat loosely their leading characteristics. But for the purpose of showing the analogy with physics we need terms of wider import than those habitually used in politics. All forms of the state imply inter-relationship in the actions of men, and action implies movement. Thus the state may be described as a configuration or arrangement of a community of men; or we may say that it implies a definite mode of motion of men—that is to say, an organized scheme of action of man on man. Political history gives an account of the gradual changes in such configurations or modes of motion of men as have possessed the quality of persistence or of stability to resist the disintegrating influence of surrounding circumstances.

In the history of a state we find this stability or power of persistence continually changing. It slowly rises to a maximum, and then declines; when it falls to nothing a revolution ensues, and a new form of government is established. This new mode of motion of men, or government, has at first but little stability, but it gradually acquires strength and permanence, until in its turn the slow decay in the power of persistence leads on to a new revolution. Such crises in political history may give rise to a condition in which the state is incapable of perpetuation by transformation. This occurs when a savage tribe nearly exterminates another and leads the few survivors into slavery; the previous form of government then becomes extinct.

Now turn to the evolution of the various forms of life. The fundamental idea in the theory of natural selection is the persistence of those types of life which are adapted to their surrounding conditions, and the elimination by extermination of the ill-adapted types. The struggle for life amongst forms possessing various degrees of adaptation to slowly varying conditions is held to explain the transmutation of species. Although a different phraseology is used when we speak of the physical world, yet the idea is essentially the same. Theories of physical evolution involve the discovery of modes of motion or configurations of matter which are capable of persistence. The physicist describes such types as stable; the biologist calls them species.

The physicist, the biologist and the historian alike watch the effect of slowly varying external conditions; they all observe the rise and decline of stability, with the consequent change of type of motion, transmutation of species or revolution.

And now after this preface I turn to astronomical theories of evolution.

The German astronomer Bode long ago propounded a simple empirical law concerning the distances at which the several planets move about the sun, and his formula embraces so large a number of cases with accuracy that we are compelled to believe that it arises in some manner from the primitive conditions of the planetary system.

The explanation of the causes which have led to this simple law as to the planetary distances presents an interesting problem, and although it is still unsolved, we may obtain some insight into its meaning by considering what may be called a working model of ideal simplicity.

Imagine, then, a sun round which there moves in a circle a single large planet. I will call this planet Jove, because it may be taken as a representative of our largest planet, Jupiter. Suppose next that a meteoric stone or small planet is projected in any perfectly arbitrary manner in the same plane in which Jove is moving; then we ask how this third body will move. It appears that under the combined attractions of the sun and Jove the meteoric stone will in general describe an orbit of extraordinary complexity, at one time moving slowly at a great distance from both the sun and Jove, at other times rushing close past one or other of them. As it grazes past Jove or the sun it may often but just escape a catastrophe, but a time will come at length when it runs its chances too fine and comes into actual collision. The individual career of the stone is then ended by absorption, and of course by far the greater chance is that it will find its Nirvana by absorption in the sun.

Next let us suppose that instead of one wandering meteoric stone or minor planet there are hundreds of them, moving initially in all conceivable directions. Since they are all supposed to be very small, their mutual attractions will be insignificant, and they will each move almost as though they were influenced only by the sun and Jove. Most of these stones will be absorbed by the sun, and the minority will collide with Jove.

When we inquire how long the career of a stone may be, we find that it depends on the direction and speed with which it is started, and that by proper adjustment the delay of the final catastrophe may be made as long as we please. Thus by making the delay indefinitely long we reach the conception of a meteoric stone which moves so as never to come into collision with either body.

There are, therefore, certain perpetual orbits in which a meteoric stone or minor planet may move forever without collision. But when such an immortal career has been discovered for our minor planet, it still remains to discover whether the slightest possible departure from the prescribed orbit will become greater and greater and ultimately lead to a collision with the sun or Jove, or whether the body will travel so as to cross and recross the exact perpetual orbit, always remaining close to it. If the slightest departure inevitably increases as time goes on, the orbit is unstable; if, on the other hand, it only leads to a slight waviness in the path described, it is stable.

We thus arrive at another distinction: there are perpetual orbits, but some, and indeed most, are unstable, and these do not offer an immortal career for a meteoric stone; and there are other perpetual orbits which are stable or persistent. The unstable ones are those which succumb in the struggle for life, and the stable ones are the species adapted to their environment.

If, then, we are given a system of a sun and large planet, together with a swarm of small bodies moving in all sorts of ways, the sun and planet will grow by accretion, gradually sweeping up the dust and rubbish of the system, and there will survive a number of small planets and satellites moving in certain definite paths. The final outcome will be an orderly planetary system in which the various orbits are arranged according to some definite law.

There is hardly room for doubt that if a complete solution for our solar system were attainable, we should find that the orbits of the existing planets and satellites are numbered amongst the stable perpetual orbits, and should thus obtain a rigorous mechanical explanation of Bode's law concerning the planetary distances.

In the first portion of my address I described the orbits in which the corpuscles move in the atoms of matter, and drew

attention to the resemblance to a planetary system. It may not, perhaps, be fanciful to imagine that some general mathematical method devised for solving a problem of cosmical evolution may find another application to miniature atomic systems, and may thus lead onward to vast developments of industrial mechanics. Science, however diverse its aims, is a whole, and men of science do well to impress on the captains of industry that they should not look askance on those branches of investigation which may seem for the moment far beyond any possibility of practical utility.

The theory which I have now explained points to the origin of the sun and planets from gradual accretions of meteoric stones, and it makes no claim to carry the story back behind the time when there was already a central condensation or sun about which there circled another condensation or planet. But more than a century ago an attempt had already been made to reconstruct the history back to a yet remoter past, and, as we shall see, this attempt was based upon quite a different supposition as to the constitution of the primitive solar system. I myself believe that the theory I have just explained, as well as that to which I am coming, contains essential elements of truth, and that the apparent discordances will some day be reconciled. The theory of which I speak is the celebrated nebular hypothesis, first suggested by the German philosopher Kant, and later restated independently and in better form by the French mathematician Laplace.

Laplace traced the origin of the solar system to a nebula or cloud of rarefied gas congregated round a central condensation which was ultimately to form the sun. The whole was slowly rotating about an axis through its center, and, under the combined influences of rotation and of the mutual attraction of the gas, it assumed a globular form, slightly flattened at the poles. The primeval globular nebula is undoubtedly a stable or persistent figure, and thus Laplace's hypothesis conforms to the general laws which I have attempted to lay down.

The nebula must have gradually cooled by radiation into space, and as it did so the gas must necessarily have lost some of its spring or elasticity, thus permitting a greater degree of condensation of the whole. The contraction led inevitably to two results: first, the central condensation became hotter; and, secondly, the speed of its rotation became faster. The accelerated rotation led to an increase in the amount of polar flattening, and the nebula at length assumed the form of a lens, or of a disk thicker in the middle than at the edges. Assuming the existence of the primitive nebula, the hypothesis may be accepted thus far as practically certain.

From this point, however, doubt and difficulty enter into the argument. It is supposed that the nebula became so much flattened that it could not subsist as a continuous aggregation of gas, and a ring of matter detached itself from the equatorial regions. The central portions of the nebula, when relieved of the excrescence, resumed the more rounded shape formerly possessed by the whole. As the cooling continued, the central portion in its turn became excessively flattened through the influence of its increased rotation; another equatorial ring then detached itself, and the whole process was repeated as before. In this way the whole nebula was fissured into a number of rings surrounding the central condensation, whose temperature must by then have reached incandescence.

Each ring then aggregated itself round some nucleus which happened to exist in its circumference, and so formed a subordinate nebula. Passing through a series of transformations, like its parent, this nebula was

finally replaced by a planet with attendant satellites.

The whole process forms a majestic picture of the history of our system. But the mechanical conditions of a rotating nebula are too complex to admit, as yet, of complete mathematical treatment; and thus, in discussing this theory, the physicist is compelled in great measure to adopt the qualitative methods of the biologist, rather than the quantitative ones which he would prefer.

Although the telescope seems to confirm the general correctness of Laplace's hypothesis, yet it is hardly too much to say that every stage in the supposed process presents to us some impossibility.

Thus, for example, the ring of Saturn seems to have suggested the theory to Laplace; but to take it as a model leads us straight to a quite fundamental difficulty. If a ring of matter ever concentrates under the influence of its mutual attraction, it can only do so round the center of gravity of the whole ring. Therefore the matter forming an approximately uniform ring, if it concentrates at all, can only fall in on the parent planet and be reabsorbed. Some external force other than the mutual attraction of the matter forming the ring, and, therefore, not provided by the theory, seems necessary to effect the supposed concentration. The only way of avoiding this difficulty is to suppose the ring to be ill-balanced or lop-sided; in this case, provided the want of balance is pronounced enough, concentration will take place round a point inside the ring but outside the planet.

However, this is not the time to pursue these considerations further, yet enough has been said to show that the nebular hypothesis can not be considered as a connected intelligible whole, however much of truth it may contain.

In the first theory which I sketched as to the origin of the sun and planets, we supposed them to grow by the accretions of meteoric wanderers in space, and this hypothesis is apparently in fundamental disagreement with the conception of Laplace, who watches the transformations of a continuous gaseous nebula. I must not pause to consider how these seemingly discordant views may be reconciled, but will merely say that I conceive both theories contain important elements of truth.

We have seen that, in order to explain the genesis of planets according to Laplace's theory, the rings must be ill-balanced or even broken. If the ring were so far from being complete as only to cover a small segment of the whole circumference, the true features of the occurrences in the births of planets and satellites might be better represented by conceiving the detached portion of matter to have been more or less globular from the first, rather than ring-shaped. Now this idea introduces us to another group of researches whereby mathematicians have sought to explain the birth of planets and satellites.

The solution of the problem of evolution involves the search for those persistent or stable forms which biologists would call species. The species of which I am now going to speak may be grouped in a family, which comprises all those various forms which a mass of rotating liquid is capable of assuming under the conjoint influences of gravitation and rotation. If the earth were formed throughout of a liquid of the same density, it would be one of the species of this family; and indeed these researches date back to the time of Newton, who was the first to explain the figures of planets.

The ideal liquid planets we are to consider must be regarded as working models of actuality, and inasmuch as the liquid is supposed to be incompressible, the conditions depart somewhat widely from those of reality. Hence, when the problem has

been solved, much uncertainty remains as to the extent to which our conclusions will be applicable to actual celestial bodies.

We begin, then, with a rotating liquid planet like the earth, which is the first stable species of our family. We next impart in imagination more rotation to this planet, and find by mathematical calculation that its power of resistance to any sort of disturbance is less than it was. In other words, its stability declines with increased rotation, and at length we reach a stage at which the stability just vanishes. At this point the shape is a transitional one, for it is the beginning of a new species with different characteristics from the first, and with a very feeble degree of stability or power of persistence. As a still further amount of rotation is imparted, the stability of the new species increases to a maximum and then declines until a new transitional shape is reached and a new species comes into existence. In this way we pass from species to species with an ever-increasing amount of rotation.

The first or planetary species has a circular equator like the earth; the second species has an oval equator, so that it is something like an egg spinning on its side on a table; in the third species we find that one of the two ends of the egg begins to swell, and that the swelling gradually becomes a well-marked protrusion or filament. Finally, the filamentous protrusion becomes bulbous at its end, and is only joined to the main mass of liquid by a gradually thinning neck. The neck at length breaks, and we are left with two separated masses which may be called planet and satellite.

In this ideal problem the successive transmutations of species are brought about by gradual additions to the amount of rotation with which the mass of liquid is endowed. It might seem as if this continuous addition to the amount of rotation were purely arbitrary and could have no counterpart in nature. But real bodies cool and contract in cooling, and I must ask you to believe that the effects of an apparently arbitrary increase of rotation may be produced by cooling.

The figures which I succeeded in drawing, by means of rigorous calculation, of the later stages of this course of evolution, are so curious as to remind one of some such phenomenon as the protrusion of a filament of protoplasm from a mass of living matter, and I suggest that we may see in this almost life-like process the counterpart of at least one form of the birth of double stars, planets and satellites.

My Cambridge colleague, Jeans, has also made an interesting contribution to the subject by attacking the far more difficult case where the rotating fluid is a compressible gas. In this case also he finds a family of types, but the conception of compressibility introduced a new set of considerations in the transitions from species to species. The problem is, however, of such difficulty that he had to rest content with results which were rather qualitative than strictly quantitative.

It can not be doubted that the supposed Laplacian sequence of events possesses a considerable element of truth, yet these latter schemes of transformation can be followed in closer detail. It seems, then, probable that both processes furnish us with crude models of reality, and that in some cases the first and in others the second is the better representative.

The moon's mass is one eightieth of that of the earth, whereas the mass of Titan, the largest satellite in the solar system, is one forty-sixth hundredths of that of Saturn. On the ground of this great difference between the relative magnitudes of all other satellites and of the moon, it is not unreasonable to suppose that the mode of separation of the moon from the earth

may also have been widely different. The theory of which I shall have next to speak claims to trace the gradual departure of the moon from an original position not far removed from the present surface of the earth. If this view is correct we may suppose that the detachment of the moon from the earth occurred as a single portion of matter, and not as a concentration of a Laplacian ring.

If a planet is covered with oceans of water and air, or if it is formed of plastic molten rock, tidal oscillations must be generated in its mobile parts by the attractions of its satellites and of the sun. Such movements must be subject to frictional resistance, and the planet's rotation will be slowly retarded by tidal friction in much the same way that a fly-wheel is gradually stopped by any external cause of friction. Since action and reaction are equal and opposite, the action of the satellites on the planet, which causes the tidal friction of which I speak, must correspond to a reaction of the planet on the motion of the satellites.

At any moment of time we may regard the system composed of the rotating planet with its attendant satellite as a stable species of motion, but the friction of the tides introduces forces which produce a continuous, although slow, transformation in the configuration. It is, then, clearly of interest to trace backwards in time the changes produced by such a continuously acting cause, and to determine the initial condition from which the system of planet and satellite must have been slowly degrading. We might also look forward, and discover whither the transformation tends.

Let us consider, then, the motion of the earth and moon revolving in company round the sun, on the supposition that the friction of the tides in the earth is the only effective cause of change. We are, in fact, to discuss a working model of the system, analogous to those of which I have so often spoken before; and it must suffice to set forth the result in its main outline as referring only to the past.

If we take the 'day,' regarding it as a period of variable length, to mean the time occupied by a single rotation of the earth on its axis; and the 'month,' likewise variable in absolute length, to mean the time occupied by the moon in a single revolution round the earth, the number of days in the month expresses the speed of the earth's rotation relatively to the speed of the moon's revolution.

Now in our retrospect both day and month are found continuously shortening; but as on the whole the month shortens much more quickly than the day, the number of days in the month also falls. We may, then, ask at once, What is the initial stage to which the gradual transformation points? I say, that on following the argument to its end the system may be traced back to a time when the day and month were identical in length, and were both only about four or five of our present hours. The identity of day and month means that the moon was always opposite to the same side of the earth; thus at the beginning the earth always presented the same face to the moon, just as the moon now always shows the same face to us. Moreover, when the month was only some four or five of our present hours in length the moon must have been only a few thousand miles from the earth's surface—a great contrast with the present distance of 240,000 miles.

It might well be argued from this conclusion alone that the moon separated from the earth more or less as a single portion of matter at a time immediately antecedent to the initial stage to which she has been traced. But there exists a yet more weighty argument favorable to this view, for it appears that the initial stage is one

in which the stability of the species of motion is tottering, so that the system presents the characteristic of a transitional form, which we have seen to denote a change of type or species in a previous case.

In discussing the transformations of a liquid planet we saw the tendency of the single mass to divide into two portions, and now we seem to reach a similar crisis from the opposite end, when in retrospect we trace back the system to two masses of unequal sizes in close proximity with one another. The argument almost carries conviction with it, but I have necessarily been compelled to pass over various doubtful points.

Time is wanting to consider other subjects worthy of notice which arise out of this problem, yet I wish to point out the fact that tidal friction is competent to explain the eccentricity of an orbit, because this conclusion has been applied in a manner to which I shall have occasion to return hereafter.

If, as has been argued, tidal friction has played so important a part in the history of the earth and moon, it might be expected that the like should be true of the other planets and satellites, and of the planets themselves in their relationship to the sun. But numerical examination of the several cases proves conclusively that this can not have been the case. The relationship of the moon to the earth is in fact quite exceptional in the solar system, and we have still to rely on such theories as that of Laplace for the explanation of the main outlines of the solar system.

I have not yet mentioned the time occupied by the sequence of events sketched out in the various schemes of cosmogony, and the question of cosmical time is a thorny and controversial one.

Our ideas are absolutely blank as to the time requisite for the evolution either according to Laplace's nebular hypothesis, or the meteoritic theory. All we can assert is that they demand enormous intervals of time as estimated in years.

The theory of tidal friction stands alone amongst these evolutionary speculations in that we can establish an exact but merely relative time-scale for every stage of the process. Although it is true that the value in years of the unit of time remains unknown, yet it is possible to determine a period in years which must be shorter than that in which the whole history is comprised. If at every moment since the birth of the moon tidal friction had always been at work in such a way as to produce the greatest possible effect, then we should find that sixty million years would be consumed in this portion of evolutionary history. The true period must be much greater, and it does not seem unreasonable to suppose that 500 to 1,000 million years may have elapsed since the birth of the moon. Such an estimate would not seem extravagant to geologists who have, in various ways, made exceedingly rough determinations of geological periods.

As far as my knowledge goes I should say that pure geology points to some period intermediate between 50 and 1,000 millions of years, the upper limit being more doubtful than the lower. Thus far we do not find anything which renders the tidal theory of evolution untenable.

But the physicists have formed estimates in other ways which, until recently, seemed to demand in the most imperative manner a far lower scale of time. According to all theories of cosmogony, the sun is a star which became heated in the process of its condensation from a condition of wide dispersion. When a meteoric stone falls into the sun the arrest of its previous motion gives rise to heat, just as the blow of a horse's shoe on a stone makes a spark. The fall of countless meteoric stones, or the

condensation of a rarefied gas, was supposed to be the sole cause of the sun's high temperature.

Since the mass of the sun is known, the total amount of the heat generated in it, in whatever mode it was formed, can be estimated with a considerable amount of precision. The heat received at the earth from the sun can also be measured with some accuracy, and hence it is a mere matter of calculation to determine how much heat the sun sends out in a year. The total heat which can have been generated in the sun divided by the annual output gives a quotient of about 20 millions. Hence it seemed to be imperatively necessary that the whole history of the solar system should be comprised within some 20 millions of years.

This argument, which is due to Helmholtz, appeared to be absolutely crushing, and for the last forty years the physicists have been accustomed to tell the geologists that they must moderate their claims. But for myself I have always believed that the geologists were more nearly correct than the physicists, notwithstanding the fact that appearances were so strongly against them.

And now, at length, relief has come to the strained relations between the two parties, for the recent marvelous discoveries in physics show that concentration of matter is not the only source from which the sun may draw its heat.

Radium is a substance which is perhaps millions of times more powerful than dynamite. Thus it is estimated that an ounce of radium would contain enough power to raise 10,000 tons a mile above the earth's surface. Another way of stating the same estimate is this: the energy needed to tow a ship of 12,000 tons a distance of six thousand sea miles at 15 knots is contained in 22 ounces of radium. The *Saxon* probably burns five or six thousand tons of coal on a voyage of approximately the same length. Other lines of argument tend in the same direction.

Now we know that the earth contains radioactive materials, and it is safe to assume that it forms in some degree a sample of the materials of the solar system; hence it is almost certain that the sun is radioactive also.

This branch of science is as yet but in its infancy, but we already see how unsafe it is to dogmatize on the potentialities of matter. It appears, then, that the physical argument is not susceptible of a greater degree of certainty than that of the geologists, and the scale of geological time remains in great measure unknown.

I have now ended my discussion of the solar system, and must pass on to the wider fields of the stellar universe.

Only a few thousand stars are visible with the unaided eye, but photography has revealed an inconceivably vast multitude of stars and nebulæ, and every improvement in that art seems to disclose yet more and more. It seems useless to consider whether the number of stars has any limit, for infinite number, space and time transcend our powers of comprehension. We must then make a virtue of necessity, and confine our attention to such more limited views as seem within our powers.

A celestial photograph looks at first like a dark sheet of paper splashed with whitewash, but further examination shows that there is method in the arrangement of the white spots. Thus there is order of some sort in the heavens, and, although no reason can be assigned for the observed arrangement in any particular case, yet it is possible to obtain general ideas as to the succession of events in stellar evolution.

Besides the stars there are numerous streaks, wisps and agglomerations of nebulosity, whose light we know to emanate

from gas. Spots of intenser light are observed in less brilliant regions; clusters of stars are sometimes imbedded in nebulosity, while in other cases each individual star of a cluster stands out clear by itself. These and other observations force on us the conviction that the wispy clouds represent the earliest stage of development, the more condensed nebulæ a later stage, and the stars themselves the last stage. This view is in agreement with the nebular hypothesis of Laplace, and we may fairly conjecture that chains and lines of stars represent preexisting streaks of nebulosity.

Change is obviously in progress everywhere, as well in each individual nebula and star as in the positions of these bodies relatively to one another. But we are unable even to form conjectures as to the tendency of the evolution which is going on. This being so, we can not expect, by considering the distribution of stars and nebulæ, to find many illustrations of the general laws of evolution which I have attempted to explain; accordingly I must confine myself to the few cases where we at least fancy ourselves able to form ideas as to the stages by which the present conditions have been reached.

Up to a few years ago there was no evidence that the law of gravitation extended to the stars, and even now there is nothing to prove the transmission of gravity from star to star. But in the neighborhood of many stars the existence of gravity is now as clearly demonstrated as within the solar system itself. The telescope has disclosed the double character of a large number of stars, and the relative motion of the pairs of companions has been observed with the same assiduity as that of the planets. When the relative orbit of a pair of binary or double stars is examined, it is found that the motion conforms exactly to those laws of Kepler which prove that the planets circle round the sun under the action of solar gravitation. A leading characteristic of all these double stars is that the two companions do not differ enormously in mass from one another. In this respect these systems present a strongly marked contrast with that of the sun, attended as it is by relatively insignificant planets.

In the earlier part of my address I showed how theory indicates that a rotating fluid body will as it cools separate into two detached masses. Mathematicians have not yet been able to carry their analysis far enough to determine the relative magnitudes of the two parts, but as far as we can see the results point to the birth of a satellite whose mass is a considerable fraction of that of its parent. Accordingly See (who devotes his attention largely to the astronomy of double stars), Alexander Roberts and others consider that what they have observed in the heavens is in agreement with the indications of theory. It thus appears that there is reason to hold that double stars have been generated by the division of primitive and more diffused single stars.

But if this theory is correct we should expect the orbit of a double star to be approximately circular; yet this is so far from being the case that the eccentricity of the orbits of many double stars exceeds by far any of the eccentricities in the solar system. Now See has pointed out that when two bodies of not very unequal masses revolve round one another in close proximity the conditions are such as to make tidal friction as efficient as possible in transforming the orbit. Hence we seem to see in tidal friction a cause which may have sufficed not only to separate the two component stars from one another, but also to render the orbit eccentric.

I have thought it best to deal very briefly with stellar astronomy in spite of the importance of the subject, because the direc-

tion of the changes in progress is in general too vague to admit of the formation of profitable theories.

We have seen that it is possible to trace the solar system back to a primitive nebula with some degree of confidence, and that there is reason to believe that the stars in general have originated in the same manner. But such primitive nebulæ stand in as much need of explanation as their stellar offspring. Thus, even if we grant the exact truth of these theories, the advance towards an explanation of the universe remains miserably slight. Man is but a microscopic being relatively to astronomical space, and he lives on a puny planet circling round a star of inferior rank. Does it not, then, seem as futile to imagine that he can discover the origin and tendency of the universe as to expect a housefly to instruct us as to the theory of the motions of the planets? And yet, so long as he shall last, he will pursue his search, and will no doubt discover many wonderful things which are still hidden. We may indeed be amazed at all that man has been able to find out, but the immeasurable magnitude of the undiscovered will throughout all time remain to humble his pride. Our children's children will still be gazing and marveling at the starry heavens, but the riddle will never be read.

G. H. DARWIN.

SCIENTIFIC BOOKS.

Index Filicum, sive Enumeratio omnium generum specierumque Filicum et Hydropteridum ab anno 1753 ad annum 1905 descriptorum adjectis synonymis principalibus, area geographica, etc. By CARL CHRISTENSEN. Copenhagen, H. Hagerup. Fasc. I, pp. 1–64, May 5, 1905; fasc. II, pp. 65–128, July 1, 1905. Price 3s. 6d. per fascicle.

The present notice of the first two fascicles of Herr Christensen's 'Index Filicum' is offered not so much in the nature of a review as with the especial purpose of calling the work to the attention of botanists and librarians, and of urging the desirability of immediate and deserved support. For, as we have been informed by the author, unless very definite encouragment is given at once in the shape of additional subscriptions it will be impossible to bring the printing to a successful conclusion. The manuscript is said to be quite complete, comprising ten or twelve parts in all, the result of years of patient, tedious effort. How unfortunate the discontinuance of publication would be is realized most by those who have borne in some measure the burden imposed by the lack of such a compendium.

The work when complete is to be under three heads: (1) A systematic enumeration of the genera of ferns, based mainly upon the treatment of Engler and Prantl; (2) an alphabetical arrangement of all valid specific names and synonyms published from 1753 to 1905, with mention of names in use among gardeners; (3) an alphabetical catalogue of references to the principal systematic literature of ferns. Of these it is undoubtedly the second which, under present conditions, will prove of greatest service to botanists; yet the first is assuredly a great desideratum, and the last will be of unusual benefit to younger students.

The want of an index has been pressing. An authoritative estimate upon the validity of the exceedingly numerous species proposed in the past is, of course, a prime consideration; but it can not be denied that this is best determined, or at least maintained, in an extended descriptive work that shall afford a general view of related species. It is too much to expect that in the present instance the treatment of species will carry the weight of monographic authority. Nevertheless, there is every indication that the author's estimate is a fair one; and, at any rate, deviation from this treatment will not detract materially from the usefulness of the work.

The main value of the volume will reside in the strictly bibliographic phase; the chief requirement being, in brief, that we may be able

to determine readily under which genera a given specific name has been applied, precisely at what place and, incidentally, by whom. The nomenclatorial confusion resulting from the widely differing schemes of classification adopted by various writers has long since become so pronounced as to offer a very serious obstacle to constructive work. What the 'Index Kewensis' has meant to botanists in general and Paris's 'Index Bryologicus' latterly to students of mosses, those who have dealt with ferns have realized only partially, hitherto, in consulting Moore's 'Index Filicum,' a work that was printed from A only midway through G in the years 1857–1862. It is true that Salamon's 'Nomenclator der Gefässkryptogamen' (1883) has been of assistance, although citations are entirely omitted; but, if we consider the activity of fern students in the last two decades alone, it becomes evident from the wide range of descriptions in periodicals—botanical and otherwise—how little security in the use of new specific names has been justified, and some idea may be gained of the difficulties that have lain in the way of ready reference to original descriptions. Christensen's 'Index' is designed to meet this condition. Judging from the character of the two parts at hand we have little doubt that opinion can not fail eventually to be substantially and deservedly favorable.

With regard to the mechanical execution of the work little but praise may be said. The typography is exceedingly well adapted to its purpose; and the method of citation is practically in accord with the common American usage, the sequence being: (1) name, (2) author, (3) title of serial, (4) series, if any, in Roman capitals, (5) volume number in bold-faced Arabic, (6) page and (7) date. In a very few cases well-known works and periodicals are abbreviated in an extreme fashion which, for the sake of avoiding cumbrous repetition seems quite justified. Accepted species stand under their proper genera in heavy-faced print; very doubtful species and those known only among gardeners in italics; synonyms in ordinary brevier. The listing of subgenera in distinctive typography is also to be strongly commended; for, although our latest American rules contain no provision that these must be taken up in the event of segregation, yet the desirability of their later use is scarcely open to question, and it is of high importance that such as are needed be used in their proper sense and that all be held available.

There is evidence of great care in citation, and of unusual effort to prevent a possible misconception as to the authorship and publication of new names and 'new combinations,' of which there are of necessity a good many. In transferring a species from one genus to another, the resulting binomial, if new, is distinctly indicated as such; not, however, in the usual way, by the phrase 'comb. nov.' but by 'C. Chr. Ind. [page] 1905'; by which means the binary name of every recognized species—whether proposed formerly or in this volume—is accompanied by citation of publication. More than a few of Dr. Christ's species are here first referred to other than their original genera; but in most, if not all, of these cases that author is credited with the new binomial (the citation being printed 'Christ in C. Chr. Ind. [page] 1905,') in recognition of assistance received in preparation of the manuscript.

Criticism of the major systematic treatment must be deferred until the appearance of the final brochures, for not until then shall we have a formal presentation of the classification adopted, nor shall we know how widely this treatment departs from that of its professed model, Engler and Prantl. There is, however, some indication of a more liberal acceptance of genera. Still, we can not but regard *Anaxetum* Schott as worthy to stand quite apart, generically distinct, from *Polypodium;* and recent studies[1] have convinced us that a more valid genus than *Adenoderris* J. Sm. is hardly to be found in the whole range of the Dryopterideæ. In the recognition of species the policy of the author has been to follow the disposition of monographers; in this way, de Vries's numerous species of *Angiopteris* are admitted, though under protest, on the ground that there has appeared no later revision.

Aside from the preparation of a modern 'Synopsis Filicum'—an undertaking so difficult and comprehensive that, under present

[1] *Botanical Gazette*, **39**: 366–369. May, 1905.

circumstances, it may scarcely be regarded with well-founded hopes of realization—there is undoubtedly no more worthy single service to be rendered students in systematic pteridology than the publication of precisely such a work as Christensen has undertaken in his 'Index Filicum.' The need of the work is undeniable; the parts already published are of high worth; the manuscript of the remainder is ready for the printer; and we can only express our hope that the necessary support shall be given—and at once—to insure the issuance of the remaining parts.

WILLIAM R. MAXON.

U. S. NATIONAL MUSEUM,
August 15, 1905.

SCIENTIFIC JOURNALS AND ARTICLES.

THE August number of *The Physical Review* contains the following articles:

A. DE FOREST PALMER: 'Thermo-electric Determination of Temperatures 0° and 200° C.'

LOUIS BEVIER, JR.: 'The Vowel A° (as in Raw), O (as in Rope), U (as in Rude).'

WM. J. RAYMOND: 'The Measurment of Inductance and Capacity by Means of the Differential Ballistic Galvanometer.'

J. B. WHITEHEAD: 'The Magnetic Effect of Electric Displacement.'

E. R. DREW: 'The Infra-red Spectrum of CO_2 and Nitrogen.'

THE contents of *The American Naturalist* for August are as follows:

PROFESSOR D. P. PENHALLOW: 'A Systematic Study of the Salicaceæ.'

J. A. CUSHMAN: 'Developmental Stages in the Lagenidæ.'

DR. B. M. DAVIS: 'Studies on the Plant Cell.'—VII.

Notes and Literature: Nature Study; Zoology, Wasps Social and Solitary, Trouessart's Catalogue Mammalium, Supplement.

SOCIETIES AND ACADEMIES.

ORGANIZATION OF A NATIONAL SOCIETY OF TEACHERS OF MATHEMATICS AND SCIENCE.

A CONFERENCE was held at Asbury Park on July 5, 1905, for the purpose of discussing the advisability of organizing a national society of teachers of mathematics and natural science. The conference was attended by thirty-seven teachers representing nearly all the larger associations of teachers of mathematics and natural science in the United States. Many letters received from teachers who were unable to be present expressed sympathy with the proposed movement.

Professor Thomas S. Fiske, of Columbia University, was elected chairman of the conference and Dr. Arthur Schultze, of the High School of Commerce of New York, was elected secretary.

There was absolute agreement in regard to the advisability of forming closer permanent relations among the associations represented, and a large majority were in favor of effecting this by means of a national association. Considerable discussion, however, arose as to whether the new society should be one of mathematical teachers only or one including also teachers of science. The western associations, for the most part including teachers of science as well as teachers of mathematics, strongly advocated a mixed organization, while the teachers from the eastern states seemed, to a considerable extent, to favor a purely mathematical society. The views urged by the western delegates prevailed, and on motion of Professor E. R. Hedrick, of the University of Missouri, a resolution was adopted to the effect that a national society of teachers of mathematics and science be organized.

The details of the organization were referred to the following executive committee: Professor Thomas S. Fiske (chairman), New York, N. Y.; Professor C. E. Comstock, Peoria, Ill.; Professor E. R. Hedrick, Columbia, Mo.; Mr. Franklin T. Jones, Cleveland, O.; Professor William H. Metzler, Syracuse, N. Y.; Mr. Edgar H. Nichols, Cambridge, Mass.

Up to the next meeting this committee is to act as council of the society and a report of its proceedings is to be published in *School Science and Mathematics*.

In the following list of associations represented at the conference the names of regularly appointed delegates are distinguished by the letter (D).

New England Mathematics Teachers Asso-

ciation.—Chas. E. Bouton, Harvard University (D); Paul Capron (D); Mr. Nichols, Brown and Nichols School, Cambridge (D).

Association of Teachers of Mathematics in the Middle States and Maryland.—John C. Bechtel; Fletcher Durell, Lawrenceville, N. J.; A. Newton Ebaugh; Miss Susan C. Lodge; Donald C. MacLaren; Wm. H. Metzler, Syracuse University (D); J. T. Rorer, Central High School, Philadelphia (D); Arthur Schultze, High School of Commerce, N. Y. (D); H. C. Whitaker.

Central Association of Science and Mathematics Teachers.—Otis W. Caldwell; Jos. V. Collins (D); C. E. Comstock (D); G. W. Greenwood (D); Charles H. Smith; Charles M. Turton; J. W. Young, Charles W. Wright.

Missouri Society of Teachers of Mathematics.—F. T. Appleby; J. S. Bryan, Central High School, St. Louis (D); H. Clay Harvey (D); E. R. Hedrick (D); B. F. Johnston; John R. Kirk; J. W. Whiteye.

Chicago and Cook County High School Teachers' Association.—Edward E. Hill (D); Fred R. Nichols (D); Chas. M. Turton (D).

Mathematical Section of Michigan School-Master's Club.—Miss Emma C. Ackermann (D).

New York State Science Association, Mathematical Department.—Glenn M. Lee.

North Eastern Ohio Center, G.A.S. and M.T.—Lemar T. Beman, Cleveland High School (D); Charles A. Marple (D).

Ohio Association of Teachers of Mathematics and Science.—Franklin T. Jones (D); Wm. McLair (D).

St. Louis Association of Science and Mathematics Teachers.—Wm. Schuyler, McKinley High School, St. Louis (D).

DISCUSSION AND CORRESPONDENCE.

THE BOLYAI PRIZE.

America will rejoice that at last Hungary is honoring herself in honoring her wonder-child, John Bolyai. His marvel diamond, the most extraordinary two dozen pages in the history of human thought, appeared in America in English before it appeared in Hungary in Magyar, proud as they are of their language; and more, the American was reproduced entire in Japan before even the original was reproduced in Hungary.

An American, not a European, was the first from outside Hungary to make the journey to Máros-Vásarhely only for John Bolyai's sake and to see there the letter in Magyar which constitutes his preemption claim and title-deed to the new universe, and to publish for the first time that letter making the date 1823 ever memorable. On its publication thus in America Charles S. Peirce wrote in *The Nation,* March 17, 1892, p. 212 in a review of Halsted's Bolyai:

> There is a winningly enthusiastic letter from Bolyai János to his father, telling him of the great step. He says: "I have discovered such magnificent things that I am myself astonished at them. It would be damage eternal if they were lost. When you see them, my father, you will yourself acknowledge it. At present I can not say more than that from nothing I have created a wholly new world."

Ten years later this letter was published in Hungary in Magyar and Latin, and now comes the establishment of the great Bolyai prize (Prix Bolyai) by the Hungarian Academy of Sciences, of which the statutes are as follows:

> 1. On the occasion of the hundredth anniversary of the birth of John Bolyai the Hungarian Academy of Sciences wishing to perpetuate the memory of this illustrious scientist, as likewise that of the profound thinker, Farkas Bolyai, his father and teacher, has decided to establish a prize to be called the Bolyai Prize. This prize, which is to consist of a commemorative medal—whose obverse will represent the academy with the view of Budapest, and whose reverse will bear an inscription—and of a sum of ten thousand crowns, shall be adjudged for the first time in 1905, then every five years, to the author of the best work in mathematics published during the five preceding years.
>
> The prize may be given to any work deemed worthy of it, whatever the language in which it be written, and in whatever form it be published.
>
> The announcement of the winner will take place during the general meeting of the academy in December.
>
> 2. In case the work of a deceased author be deemed worthy the prize, this shall be given to his heirs.
>
> 3. The third section of the academy, section of

sciences, is entrusted with constituting, at its March meeting, a committee composed of two home and two foreign members, whose duty it shall be to judge of the value of the works. The committee will meet at Budapest in the first fortnight of October, and name from their number a president and a reporter.

In case of a tie the president's vote is preponderant.

It shall be the duty of the reporter to present a detailed report on the committee's decision.

This report is to be read at the general meeting of the Academy of Sciences the day the prize is adjudged.

4. The works of authors on the committee are excluded from the competition, and they are not to be mentioned in the committee's report.

5. The foreign members designated as part of the committee and who, participating in the deliberations, will spend some days at Budapest, shall receive a compensation of 1,000 crowns. The honorarium accorded to the reporter for his work is fixed at 300 crowns.

6. The report is to be published in the journal 'Akadémiai Értesitö.' The Hungarian Academy of Sciences will publish this report abroad, and will make it known to all the associated academies.

In accordance with the above statutes, in the course of this present year the Hungarian Academy of Sciences will confer for the first time the Bolyai Prize, consisting of a medal and ten thousand crowns.

The commission constituted by the academy from its members and endowed with the powers of a jury consists of Gaston Darboux (Paris), Felix Klein (Göttingen), Julius König (Budapest), Gustav Rados (Budapest). The deliberations of this commission will be held this October in Budapest.

If I may be forgiven for a bit of prophecy, I venture to predict the prize goes to Poincaré.

GEORGE BRUCE HALSTED.

KENYON COLLEGE,
GAMBIER, OHIO.

SPECIAL ARTICLES.

ON THE PROBABLE ORIGIN OF CERTAIN BIRDS.

IT is my purpose to examine in this article the status of nine kinds of birds that have been recorded from North America, and one that has been taken in southern Europe, and to discuss in some detail their relationship and probable origin.

Appended to the 'Check-list of North American Birds' published by the American Ornithologists' Union there is a 'Hypothetical List' consisting of twenty-eight different birds which, for various reasons, have an uncertain status in the bird fauna of the region for which the list is given. Of these twenty-eight birds I shall consider nine, as from the evidence at hand it would appear that together they throw much light on some hitherto obscure problems. The list includes Cooper's sandpiper, *Tringa cooperi* Baird; Brewster's linnet, *Acanthis brewsterii* Ridgway; Townsend's bunting, *Spiza townsendii* (Audubon); Lawrence's warbler, *Helminthophila lawrencii* (Herrick); Brewster's warbler, *Helminthophila leucobronchialis* (Brewster); Carbonated warbler, *Dendroica carbonata* (Audubon); Blue Mountain warbler, *Dendroica montana* (Wilson); Small-headed warbler, *Wilsonia microcephala* (Ridgway); Cuvier's kinglet, *Regulus cuvierii* Audubon.

Of these nine kinds of birds seven either are represented by single individuals or are known only from figures and descriptions in the works of Audubon and Wilson. On the other hand, the two remaining birds of this series are known by numerous specimens, and my reasons for including them will be presented as each is considered in detail.

It seems essential at this point to call attention to the fact that a number of these birds were discovered at a time when field naturalists were not nearly so numerous as at the present day, and that there may be no doubt as to the reality of at least some of these forms, a number of the types still exist, as will presently be shown.

COOPER'S SANDPIPER, TRINGA COOPERI BAIRD.

Cooper's sandpiper is known from a single individual that was taken on Long Island in May, 1833. The type is still in the National Museum at Washington. The evident relationship of this bird to the knot, *Tringa canutus* Linnæus, is at once apparent to a student, and even an untrained eye might readily distinguish their similarity. For the

original account of the type of this species the reader is referred to 'The Birds of North America,' Baird, 1858, page 716.

BREWSTER'S LINNET, ACANTHIS BREWSTERII RIDGWAY.

The type specimen of Brewster's linnet was taken by Mr. William Brewster at Waltham, Mass., on November 1, 1870. The bird is a female. The type still exists in the collection of Mr. Brewster at Cambridge, and no other individual of this kind is known. In appearance the bird differs from other members of the genus in which it has been placed by Mr. Ridgway chiefly in lacking the red spot on top of the head and the dusky spot on the chin characteristic of the adults, especially the males of the genus *Acanthis*. Therefore, the exact relationship of this bird is somewhat obscure, though its generic status has not been questioned. For the original description of this species the reader is referred to the *American Naturalist* of July, 1872, page 433.

TOWNSEND'S BUNTING, SPIZA TOWNSENDII (AUDUBON).

On May 11, 1833, Mr. J. K. Townsend, obtained, while collecting, the type specimen on which this form is based. It is an adult male, and remains unique. The relationship of this bird is obvious; it can only be regarded as the close ally of the dickcissel, *Spiza americana* (Gmelin). (Cf. Audubon's 'Ornithological Biography,' Vol. II., p. 183, 1834.)

Commenting on the status of this bird the Committee of the Ornithologists' Union say: 'Its peculiarities can not be accounted for by hybridism nor probably by individual variation.'[1]

CARBONATED WARBLER, DENDROICA CARBONATA (AUDUBON).

This bird is known only from Audubon's colored plate and his description of two specimens killed near Henderson in Kentucky in May, 1811. The birds were probably both males. Audubon's account of the event may be found in the 'Ornithological Biography,' Vol. I., p. 308, pl. 60, 1831.

BLUE MOUNTAIN WARBLER, DENDROICA MONTANA (WILSON).

The Blue Mountain warbler is only known from the works of Wilson and Audubon. The specimens on which they based their descriptions were taken in the Blue Ridge Mountains of Virginia. The bird was figured, but no specimens are at present known. (Cf. Wilson, 'American Ornithology,' Vol. V., p. 113, pl. 44, fig. 2, 1812.)

SMALL-HEADED WARBLER, WILSONIA MICROCEPHALA (RIDGWAY).[2]

This again is one of the species described by both Wilson and Audubon. It is said to have been taken in points so widely separated as New Jersey and Kentucky, but is only known by the colored plates and the descriptions made by the above naturalists. It does not seem probable that with all the careful detailed work that has been done in both regions during the last fifty years the small-headed warbler is till extant. The bird is so widely different from any of its congeners as to make confusion with them impossible, nor has the theory of hybridity been advanced to account for this supposed species. There then remain the two hypotheses as to the status of *Wilsonia microcephala;* either the individuals which came under the observation of Wilson and Audubon were the last survivors of this species which was dying out and has become extinct, or these birds were *'mutations'* that occurred ephemerally and did not flourish, but died out almost immediately.[3]

CUVIER'S KINGLET, REGULUS CUVIERII AUDUBON.

On June 8, 1812, Audubon obtained on the banks of the Schuylkill River, at a place called Flatland Ford, in Pennsylvania, the only specimen of Cuvier's kinglet known. If

[1] 'A. O. U. Check list N. A. Birds,' 2d edition, p. 331, 1895.

[2] *Muscicapa minuta* Wilson (cf. *Am. Orn.*, Wilson,, Vol. VI., p. 62, 1812, pl. 1, fig. 5, nec. Gmelin, 1788).

[3] Cf. Ridgway, *Pro. U. S. Nat. Mus.*, Vol. VIII., p. 354, 1885.

the bird was preserved the specimen has probably been either lost or destroyed, and we know it only by the admirable plate which Audubon left and by his description of the little bird.[4]

It does not seem probable that other individuals of this species could have escaped the notice of the many competent naturalists who have worked in the area in question since Audubon's time.

The affinities of this little bird appear to be with *Regulus satrapa* and it now seems probable that this was a veritable '*mutation*' that did not survive.

In dealing with the foregoing seven species I have tried to find the simplest solution to account for their presence. In view of the light thrown by the succeeding examples and the data regarding the foregoing, already given, the law of parsimony compels me to consider these forms as mutations (which were not perpetuated) from *species still existing* which I have, in most cases, been able to indicate.

We have now to consider the two remaining birds making up the nine North American species. They are Brewster's warbler and Lawrence's warbler.

BREWSTER'S WARBLER, HELMINTHOPHILA LEUCOBRONCHIALIS (BREWSTER).

The type specimen of *Helminthophila leucobronchialis*[5] was taken by Mr. William Brewster at Newtonville, Mass., on May 18, 1870. The bird was a male. It was not until April, 1876, some six years afterward, that the bird was named and described by Mr. Brewster. A second specimen[6] of this species was obtained on May 12, 1877, near Clifton, Pa. It was also a male. The third recorded individual[7] was killed long before this and was discovered in the collection of the Philadelphia Academy of Natural Sciences labeled 'J. C., October 20, 1862.' The specimen had no history, but was labeled in the handwriting of John Cassin and presumably was at one time in his collection.

By the year 1885 twenty-two[8] individuals

[4] Cf. Audubon, 'Onithological Biography,' Vol. I., p. 288, pl. 55, 1832.

[5] 'Description of a New Species of *Helminthophaga*,' by Wm. Brewster, *Bulletin of the Nuttall Ornithological Club*, Vol. I., No. 1, p. 1, 1876. Original description with colored plate.

[6] 'Capture of a Second Specimen of *Helminthophaga leucobronchialis*,' by Spencer Trotter, Philadelphia, Pa., *ibid.*, Vol. II., No. 3, pp. 79–80, 1877. Mr. Trotter records in a note the capture of *H. leucobronchialis*, by Mr. Christopher D. Wood, on May 12, 1877, near Clifton, Delaware Co., Pa. The bird was a male. It was identical in appearance with the type.

[7] 'A Third Specimen of *Helminthophaga leucobronchialis*,' by Spencer Trotter, Philadelphia, Pa., *ibid.*, Vol. III., No. 1, p. 44, 1878. Mr. Trotter discovered a specimen of *H. leucobronchialis* in the collection of the Philadelphia Academy of Natural Sciences, labeled 'J. C., 20 October, 1862,' and also what he made out to be 'Not from Bell.' No sex was indicated. The bird closely resembled the type.

'Some Light on the History of a Rare Bird,' by Spencer Trotter, Philadelphia, Pa., *ibid.*, Vol. IV., No. 2, p. 59, 1879. Mr. Trotter by correspondence discovers that Mr. Bell, a taxidermist in New York, and also a naturalist, who at times assisted Audubon, recalled the fact that in the spring about 1832, at Rockland, N. Y., he shot what he supposed was a young golden-wing warbler, *H. chysoptera*. He finally sold it to a man in Philadelphia. Mr. Trotter concludes that this is the so-called Cassin specimen. Also that the words 'not from Bell' might mean 'note from Bell.'

[8] 'The White-Throated Warbler (*Helminthophaga leucobronchialis*) in Connecticut,' by William Brewster, Cambridge, Mass., *ibid.*, Vol. III., No. 2, p. 99, 1878. Mr. Brewster identifies a fourth specimen collected at Wauregan, Conn., May 25, 1875. The sex was not determined. The bird closely resembled the type. At this time Mr. Brewster regards 'The validity of this distinctly characterized species' 'as established.'

'Capture of a Fifth Specimen of the White-throated Warbler (*Helminthophaga leucobronchialis*),' by William Brewster, Cambridge, Mass., *ibid.*, Vol. III., No. 4, p. 199, 1878. This bird was taken at Suffield, Conn., July 3, 1875. This is an adult male in worn plumage. It differs somewhat from the type, chiefly in being washed with pale yellow on the pectoral region. The yellow on the wings is also restricted and the wing bars are not almost confluent as in the type.

of *H. leucobronchialis* had been secured by different collectors at various points in southern New England, the lower Hudson River Valley, New Jersey, Pennsylvania, Virginia

'Record of Additional Specimens of the White-Throated Warbler (*Helminthophaga leucobronchialis*),' by H. A. Purdie, Newton, Mass., *ibid.*, Vol. IV., No. 3, p. 184, 1879. Mr. Purdie describes four additional birds; a typical specimen collected in Hudson, Mass., in May or June, 1858. This specimen is in the possession of Williams College, Williamstown, Mass. A second bird is from Portland, Conn., where it was collected on May 22, 1875. This is a male and has a decided blotch of yellow on the breast, and a general suffusion of the lower parts with a fainter wash of this shade. There is also a slight suffusion of this color on the upper parts. The third specimen was taken at Saybrook, Conn., and was thus written of by J. N. Clark who collected it: "Took a fine male *H. leucobronchialis*, May 30 (1879); an exceptional specimen, with a patch of bright yellow across the breast from the bend of the wings. Thought it was *pinus* when I fired; notes and habits the same." A fourth bird was shot by Mr. Gunn, in Ottawa Co., Mich., and described as '*H. Gunnii*, Gibbs,' in a local newspaper. The bird is a female and was taken on May 25, 1879. It is characterized by a bright yellow breast, the color extending as far down as the abdomen and on the flanks; its crown is particularly brilliant. Mr. Robert Ridgway subsequently identified this bird as *H. leucobronchialis*, Brewster. He also comments on its unusual coloration, but says it 'is in all essential respects like the type' and further that with this 'seventh specimen thus far collected the validity of *H. leucobronchialis* may be considered as established beyond question.' (Cf. *Bull. Nutt. Orn. Club*, Vol. IV., No. 4, p. 233, 1879.)

'*Helminthophaga leucobronchialis* in New York,' by A. K. Fisher, M.D., Sing Sing, N. Y., *ibid.*, Vol. IV., No. 4, p. 234, 1879. Records an adult male taken at Sing Sing, N. Y., on August 24, 1879. The bird had a band of yellow across the breast and a slight suffusion of pale yellow on the throat; the wing bars were '*whitish*, whiter even than *H. pinus*. The back is that of a typical *H. leucobronchialis*.'

'Two More Specimens of *Helminthophaga leucobronchialis* from Sing Sing, N. Y.,' by A. K. Fisher, M.D., Sing Sing, N. Y., *ibid.*, Vol. VI., No. 4, p. 245, 1881. Records the capture of a probable female 'having a black auricular patch.' This bird was taken on July 24, 1881, near Sing Sing, N. Y. Also a specimen from the same region August 3, 1881, sex not given, 'with a yellow pectoral band, * * * the wing-bands were normal; yellow, not white.'

'Another Example of *Helminthophaga leucobronchialis* from Connecticut,' by John H. Sage, Portland, Conn. (and a footnote by William Brewster), *Auk*, Vol. I., No. 1, p. 91, 1884. Records the capture of a female at Deep River, Conn., on May 18, 1880. Mr. Brewster says in the footnote: 'It differs from the type * * * in having the yellow of the forehead partially obscured, * * * in the unusual restriction of the wing-bands, and in the generally immature appearance of the plumage.' These characteristics, he says, 'are just what would be expected in the female of this species.'

'Occurrence of *Helminthophila leucobronchialis* in Virginia,' by William Palmer, Smithsonian Institution, Washington, D. C., *Auk*, Vol. II., No. 3, p. 304, 1885. Records the capture of a male near Fort Meyer, Arlington, Va. The specimen is typical.

'A Specimen of *Helminthophila leucobronchialis* in New Jersey,' by C. B. Riker, New York City, *Auk*, Vol. II., No. 4, p. 378, 1885. Records a male collected at Maplewood, Essex Co., N. J., May 11, 1883. First record for the state. Very gray on the back, this bird has an indistinct yellow breast band and whitish wing bars much as in *pinus*, very conspicuously separated.

For change of generic name cf. Ridgway, *Bull. Nutt. Orn. Club*, Vol. VII., No. 1, p. 53, 1882.

'Capture of Two More Specimens of *Helminthophila leucobronchialis* at Sing Sing, New York,' by A. K. Fisher, M.D., Sing Sing, N. Y., *Auk*, Vol. II., No. 4, p. 378, 1885. Records the capture of two specimens at Sing Sing, N. Y., on August 11, 1883. "The under parts of both specimens are much more deeply suffused with yellow than is the case in any of my other three specimens; in fact, the yellow on one is evenly distributed over the entire under surface, but is not so deep as in *Helminthophila pinus*."

Cf. Ridgway, *Auk*, Vol. II., No. 4, pp. 359–363, October, 1885.

Cf. Thurber, *Auk*, Vol. III., No. 3, p. 411, 1886.

Remark. These are all the *recorded* individuals up to the end of 1885, but I have reason to think that there are a number of known specimens that were not recorded, and which were taken between 1878 and 1885.

and Michigan. However, the birds are most common about the lower reaches of the Connecticut and Hudson rivers. At the present writing I have no doubt that in all the collections in the country there are at least 150 individuals of *H. leucobronchialis* and, moreover, it is entirely possible, at the proper season and locality, to observe these birds annually.

Again I must insist upon the importance of considering carefully the history of the appearance of this and other probable 'mutations.' It is not likely that a form or kind of bird so common as *H. leucobronchialis* is at the present time, and ranging over as large an area as from Pennsylvania to Massachusetts and from Virginia to Michigan, should remain unknown to the earlier ornithologists, such keen field naturalists as Audubon and Wilson, Baird, Lawrence, Coues and Prentiss. Nuttall made careful and prolonged study of birds in the region where Mr. Brewster collected the type. Yet none of these close observers and good collectors either recorded or secured an individual of this kind. Clearly then, the presumption is that this bird could not have been so common early in the last century as it is now, if indeed it existed at all at that time. Nor does it seem that the theory of hybridity[9] is supported when we consider the vast number of known specimens already in collections and the fact that it is possible to observe living specimens, as I have indicated, each year. I am aware that many

[9] 'On the Relationship of *Helminthophaga leucobronchialis*, Brewster, and *Helminthophaga lawrencei*, Herrick; with some Conjectures Respecting Certain other North American Birds,' by William Brewster, *Bull. Nutt. Orn. Club*, Vol. VI., No. 4, pp. 218–225, 1881. Basing his hypothesis upon similarity of color and marking, Brewster considers these birds hybrids and says: 'Taken as a whole, this series (of seven specimens) perfectly connects *leucobronchialis* with *pinus*, as well as showing an extension of the former towards *chrysoptera*.'

'*Helminthophila leucobronchialis*,' by Robert Ridgway, *Auk*, Vol. II., No. 4, pp. 359–363, 1885. Assumes *Helminthophila leucobronchialis* to be a distinct species which hybridizes with its allies, thus accounting for the number of aberrant specimens.

'*Helminthophila leucobronchialis* in New Jersey,' by E. Carlton Thurber, Morristown, N. J., *Auk*, Vol. III., No. 3, p. 411, 1886. Records the capture by Mr. Auguste Blanchet of a specimen about ten miles from Morristown, in May, 1859. He says: 'The whole plumage resembles somewhat that of the female *H. chysoptera*, but the grayish on the breast is not so deep.' Mr. Thurber regards this bird as a hybrid.

'An Interesting Specimen of *Helminthophila*,' by William Brewster, Cambridge, Mass., *Auk*, Vol. III., No. 3, pp. 411–412, 1886. Records another specimen taken by Mr. Frank Blanchet two miles from Morristown, N. J., on May 15, 1884. The sex was not determined. Mr. Brewster writes of this specimen that it '* * * is apparently a hybrid between the hybrid *H. lawrencei* and the typical *H. pinus*.' After describing the bird with much detail he adds: "In briefer terms, this interesting bird may be said to be about intermediate in color and markings between typical *pinus*, with its short narrow eye-stripe and uniformly yellow underparts, and the so-called *H. lawrencei*, which has a broad black patch extending from the bill through and beyond the eye, and the chin, throat and forepart of the breast solidly black. It forms an important link in the chain of evidence supporting my theory (*Bull. Nutt. Orn. Club*, Vol. VI., No. 4, pp. 218–225, 1881) that *H. pinus* and *H. chrysoptera* frequently interbreed, and that their offspring perpetuate a variously characterized hybrid stock by breeding back into one or the other parent strains."

'The Significance of Certain Phases in the Genus *Helminthophila*,' by Spencer Trotter, M.D., *Auk*, Vol. IV., No. 4, pp. 307–310, 1887. Accepting the theory of hybridity, Dr. Trotter believes, because of its apparent common occurrence, as represented in the many specimens of *H. leucobronchialis*, that it indicates the degeneracy of the species producing this hybrid. He concludes, therefore, that the extinction of *chrysoptera* and *pinus* is in process, and perhaps imminent.

'Notes on Birds Observed in the Vicinity of Englewood, New Jersey,' by Frank M. Chapman, *Auk*, Vol. VI., No. 4, pp. 302–305, 1889. Mr. Chapman writes of *leucobronchialis* as 'this puzzling hybrid.'

Cf. Ridgway, 'Manual of North American Birds,' 1896, footnotes on page 486. Mr. Ridgway advances the dichroic theory plus hybridity to account for *lawrencei* and *leucobronchialis*.

seen either *Helminthophila leucobronchialis* or *H. pinus* attending young which they supposed to be *H. leucobronchialis*. And it is also on record that a parent *leucobronchialis* was observed with two young, feeding them. This bird was observed with these young ones two different days in the same locality. But two young composed the brood, and Dr. Bishop, who saw and collected them, writes: 'A careful search on both days through the adjacent country failed to disclose any other member of the genus *Helminthophila*.' He believed the parent bird to be a female and also concluded, though the two young were still in the down plumage of nestlings, from the final feathering that showed through, that: 'One, and probable that the other, would have become a typical specimen of *H. pinus*.'

These facts[10] would in themselves seem to controvert the theory of hybridity, for, though hybrids do occur among wild birds, they can

[10] 'Notes on *Helminthophila leucobronchialis*,' by Edwin H. Eames, Seymour, Conn., *Auk*, Vol. V., No. 4, pp. 427–428, 1888. Records the capture or observation of six adult and several young in a brood, between May 26, 1888, and June 22, of the same year, near Seymour, Conn. From this series of notes I quote Mr. Eames, writing of *H. leucobronchialis*, the date being June 3: "At last with more eagerness than usual it descended, and disappeared in the bushes (an unusual occurrence), where it apparently took possession of its nest, as in less than half a minute thereafter an *H. pinus*, the first I had seen in the neighborhood, flew hastily from about the same place. This occurred at about sunset, and between that and dark *leucobronchialis* did not again appear in sight. I had previously had it in view, or could hear its song, almost continuously. On several days following I searched this thicket thoroughly, as it seemed, and once succeeded in flushing a *pinus*, but could not even then find its nest. In company with *pinus*, *leucobronchialis* cautiously approached and surveyed me for a short time, then departed with no apparent misgivings. At all other times *leucobronchialis* was near by and always reconnoitered the track of my careful search when I had moved to some distance, then, apparently satisfied, pursued its avocations as before. I was not able to visit the spot again until June 17, and neither then nor since have I found this *leucobronchialis*, but I did find a brood of several young being fed by an *H. pinus*, possibly the result of a union between the two. These two birds were the only ones of the genus which I had at any time detected in the locality."

'Notes on the Blue-winged Warbler and its Allies (*Helminthophila pinus*, *H. leucobronchialis*, *H. lawrencei* and *H. chrysoptera*) in Connecticut,' by Edwin H. Eames, *Auk*, Vol. VI., No. 4, pp. 305–310, 1889. Speaking of the comparative abundance of *H. chrysoptera* and *H. leucobronchialis* in southern Connecticut, Mr. Eames writes: "Of *H. chrysoptera* but little can be said, as it is properly considered a rather rare bird here, and our yearly records are but few, usually less than half a dozen."

"The beautiful *H. leucobronchialis* is much (?) more common than the latter (*chrysoptera*) and is eagerly sought after by most of our collectors, latterly with good success considering its former (supposed) rarity." During the spring of 1889 he procured five specimens, and recorded the breeding of *leucobronchialis* as follows: "Mr. C. K. Averill, Jr., of this city, found a *leucobronchialis* early in June. * * * June 24 I accompanied him to the place and we soon had the pleasure of watching the bird at shorter range than I think has fallen to the lot of others, *i. e.*, three to ten feet. * * * It came to the same conspicuous clump of bushes and briar many times, with from one to five minutes' intermission, each time with one or more small worms, about three quarters of an inch long, first reconnoitering, then cautiously approaching, and again hastily leaving a part of this clump of bushes not over two feet in extent. We failed to discover the identity of the object of its cares, but I have reason to believe it was a young cowbird. The rest of this brood was being fed by the only *H. pinus* (a female) to be found in the neighborhood. * * * They showed a marked general similarity to the young of *pinus*. I shot this male *leucobronchialis* August 8 and also one of the young, carefully observing that the others were similar to the one killed, which was altogether too familiar with the adult bird to allow a possibility of doubt concerning its male parent.

"In this, as in many other species of our smaller birds, such an affection is shown for the haunts occupied during the nesting period that they rarely leave them until after moulting, or even till the commencement of the fall migration. In the above case I never failed to find the birds

be considered at best as only casual, and the infertility of hybrids, especially among the higher animals, is too well known to need further comment here. If it be conceded then as improbable that over one hundred cases of wild hybridity have occurred between *H. chrysoptera* and *H. pinus,* only one other conclusion can be reached, namely, that from one of these warblers (probably *H. pinus*) there began to occur 'mutations' that have increased in geometrical progression and have finally grown sufficient in number to become themselves a parent stock, though it seems probable that the 'mutations' are still occurring from the ancestral stock, as witness the observations of good field ornithologists alluded to above, who say they have seen *H. pinus* feeding young which they supposed to be *H. leucobronchialis.* They supposed the young to be *H. leucobronchialis,* because in every case one of the parents was *H. leucobronchialis,* but, on the other hand, in every case, one of the parents was an *H. pinus.* Now the cases where such conditions have prevailed are *five* in number. It is significant that while the generally accepted hypothesis to account for the origin of *H. leucobronchialis,* is that *H. chrysoptera* has crossed with *H. pinus,* the result being a hybrid, *H. leucobronchialis,* yet in no case has any naturalist asserted that he has seen *H. chrysoptera* feeding young supposed to be *H. leucobronchialis.*

I am aware that there are two cases[11] of the

within the bounds of a two-acre tract of land."

'On the Breeding of *Helminthophila pinus* with *H. leucobronchialis* at Englewood, New Jersey,' by Frank M. Chapman, American Museum of Natural History, New York City, *Auk*, Vol. IX., No. 3, pp. 302–303, 1892. Record of typical male *H. pinus*, breeding with non-typical female *H. leucobronchialis.* Description of nest and eggs. This pair of birds deserted the nest and further observation could not be made.

'Notes from Connecticut,' by E. H. Eames, Bridgeport, Conn., *Auk*, Vol. X., No. 1, pp. 89–90, 1893. Mr. Eames writes: "* * * while on the other side and within a stone's throw a beautiful Brewster's warbler spent the greater part of his time. The latter after patient watching revealed his mate, a blue-winged warbler, and a nest in course of construction. * * * When seen again, on June 14, it contained four eggs, two of which were cowbirds', which were removed. Those remaining brought forth a pair of birds that, as they left the nest, could not be distinguished from normal young of the female parent, as would be expected, whatever the color of the male."

'*Helminthophila leucobronchialis*,' by Louis B. Bishop, M.D., New Haven, Conn., *Auk*, Vol. XI., No. 1, pp. 79–80, 1894. "On July 1, 1893, I found an adult *H. leucobronchialis* with two young in a small tract of alder swamp and woodland of North Haven, Conn. They were little disturbed at my presence, and I watched them carefully for some time. The adult fed both young at short intervals, leaving little doubt of its relationship to them. On July 4 they were still in the same locality, and I collected all three. Possibly the remainder of the family had been killed, as a careful search on both days through the adjacent country failed to disclose any other member of the genus *Helminthophila.*

"Decomposition was so far advanced before I could prepare the adult that I was unable to determine its sex. The fact that it never sang while I was watching it, together with the generally dull color of its plumage, lead me to think it a female.

"Unfortunately both of the young were still principally in the olive, downy plumage of nestlings, but enough of the final feathering had appeared on the throat, breast and upper parts to make it certain that one, and probable that the other would have become a typical specimen of *H. pinus.* The wing-bars of the young differ, being in the most mature specimen narrow and almost white, and in the other broader and light yellow."

[11] 'Evidence concerning the Interbreeding of *Helminthophila chrysoptera* and *H. pinus*,' by A. K. Fisher, M.D., Sing Sing, N. Y. "On July 4, 1885, while collecting specimens in a piece of woods underlaid by a scattering undergrowth, I came upon a female golden-winged warbler busily engaged in collecting insects. As I stood watching her she flew to a neighboring cedar tree and commenced to feed a young bird. I immediately shot and killed the latter as the female flew away. The noise of the discharge started another young bird from some bushes near by, and as it flew the female flew and alighted near it. Just as I was on the point of firing they started,

mating of *H. chrysoptera* with *H. pinus,* but in the first of these, as will presently appear, the male parent is hypothetical. It is to be noted that in the first case Dr. Fisher found a female *H. chrysoptera* feeding a young *H. pinus* in a cedar tree. Upon the shooting of this fledgling the old female flew away; the shot startled another young bird from some bushes near by, and as it flew the female also flew and alighted near it and was then shot. The young bird that was killed 'was the exact counterpart of the blue-winged yellow warbler' (*H. pinus*), while the second bird carefully observed resembled the adult bird that was shot, and was, therefore, apparently a young *H. chrysoptera.* No male parent was seen nor were any other young observed.

Granting that both of these fledglings were the progeny of the bird seen feeding one and associated with the other, and also granting that the unknown male parent of both these young birds was *H. pinus,* neither of the young was *H. leucobronchialis,* the hybrid which it is asserted is the result of such a union.

The second case which is recorded by Mr. Sage goes on to state that a male *H. pinus* and a female *H. chrysoptera* were discovered feeding five young in a nest; these birds flew out of the nest on being approached, whereupon all seven were collected. The author says that the male was typical *pinus* and the female typical *chrysoptera,* 'strongly marked with yellow below.' The five young proved to be two males and three females and 'are all similar,' being olive green in color, becoming olive yellow on the abdomen and having the wings like young *pinus.* Surely these young are not *leucobronchialis,* and while the interbreeding of *chrysoptera* and *pinus* is hereby thoroughly established as a rare and casual occurrence, these hybrid young, the result of this union, so far as I can perceive, are a direct refutation of the hybrid theory, which attempts to account for the origin of *H. leucobronchialis.*

On the other hand, we have direct evidence that both *H. leucobronchialis* and the rarer *H. lawrencei* have mated and bred and reared young with *H. pinus.*

In view of the foregoing facts, I am of the opinion that in *H. leucobronchialis* and in *H. lawrencei,* presently to be considered, we have examples of two separate and distinct 'mutations' from a common parent stock or species. That is, I believe that *H. pinus,* early in the last century became unstable as

and I succeeded in wounding the female only and had to follow and kill her with a second shot. On my return to the place where I first shot at her, I could not find the young one, nor did a careful search disclose it. In advancing for a nearer shot I had a good opportunity of seeing the young bird: it closely resembled its mother in appearance and had no yellow on the breast, whereas the one killed was the exact counterpart of the young of the blue-winged yellow warbler, with its yellow breast and white wing-bars. In all probability the father of this interesting family was a specimen of *Helminthophila pinus.*"

This is the entire account of the incident. (W. E. D. S.)

'The Interbreeding of *Helminthophila pinus* and *H. chrysoptera,*' by John H. Sage, Portland, Conn. "On June 13, 1889, Mr. Samuel Robinson, who has collected with me here for the past fifteen years, noticed a male *Helminthophila pinus,* with food in its bill, fly and disappear at the foot of a small alder. A female *Helminthophila chrysoptera* soon appeared, also with food, and was lost to sight at the same spot as the other bird. On going to the locality five young birds flew from the nest and alighted on the bushes in the immediate vicinity. Both parent birds were soon feeding the young again. He shot the old birds and secured all the young, which, together with the nest, are in my cabinet.

"* * * The male (*pinus*) is a very bright specimen with white wing-bars, edged with yellow. The female (*chrysoptera*) is strongly marked with yellow below, the wing-bars being exceptionally rich with the same color.

"The young, two males and three females, are all similar, and have the head, neck, chest, sides and back olive-green. Abdomen olive-yellow. Remiges like adult *pinus.* Two conspicuous wing-bars of light olive edged with yellow."

This is the entire account of the incident, except a description of the locality, the nest and its situation. (W. E. D. S.)

a species[12] and began to throw off what must be considered as 'mutants,' taking de Vries's definition of the word. In other words, *H. pinus* is alone responsible and is the direct ancestor of both *H. leucobronchialis* and *H. lawrencei;* that these 'mutants' have up to the present time generally bred back into the parent stock, and that in so doing the instability of *H. pinus* has increased geometrically with the constant result of the increasing number of both kinds of 'mutants.'

LAWRENCE'S WARBLER, HELMINTHOPHILA LAWRENCEI (HERRICK).

Previous to Mr. Brewster's description of *Helminthophila leucobronchialis,* Herrick described[13] a bird which he named *Helminthophila lawrencei.* The affinities of this species are evidently with *H. pinus,* which it resembles in many ways, but from which it differs in being bright olive green above, and in having the ear coverts black and an area on the throat the same color.

At the time of this writing, between twenty and twenty-five[14] specimens are known, there being some confusion as to three of them. These birds are generally believed to be hybrids or crosses between *H. pinus* and *H. chrysoptera.*[15] But no one has ever intimated that they have seen *lawrencei* mated with *chrysoptera,* or *chrysoptera* feeding young

[12] Cf. Bishop, *Auk*, Vol. XXII., No. 1, pp. 21–24, 1905. "In southern Connecticut there are three distinct forms of the blue-winged warbler (*H. pinus*) taking males alone into consideration—the ordinary form with rich gamboge-yellow lower parts, white wing-bars and bright olive-green back; a second form like the last but with gamboge-yellow wing-patch, resembling the golden-winged (*H. chrysoptera*), which is much the rarest; and third, a form with pale yellow lower parts, much paler back, and with usually yellow wing-bars; and between the three occur all sorts if intermediates.

[13] *Proceedings of the Academy of Natural Sciences of Philadelphia*, p. 220, plate 15, 1874.

[14] 'Description of a New Species of *Helminthophaga*, by Harold Herrick, *Proceedings of the Academy of Natural Sciences of Philadelphia,* 1874, p. 220, pl. 15.

'Capture of a Second Specimen of *Helminthophaga lawrencei,*' by Harold Herrick, *Bull. Nutt. Orn. Club*, Vol. II., No. 1, pp. 19–20, 1877. Records the capture of a second specimen which Mr. George N. Lawrence obtained from a dealer who told him that it was taken near Hoboken, N. J., in the spring of 1876. It was apparently a male and closely resembled the type.

'A Third Specimen of Lawrence's Warbler,' by Clark G. Voorhees, New York City, *Auk*, Vol. V., No. 4, p. 427, 1888. Records the capture at Rye, Westchester Co., N. Y., on August 31, 1888, of an adult male. The third known specimen.

'*Helminthophila pinus, H. chrysoptera, H. leucobronchialis* and *H. lawrencei* in Connecticut in the Spring of 1888,' by Louis B. Bishop, M.D., New Haven, Conn., *Auk*, Vol. VI., No. 2, pp. 192–193, 1889. Records the capture of three specimens: a female at New Haven, May 21, a female at Stamford, May 23, and a male at the same place on May 25. This makes six known specimens.

Cf. *op. cit.*, *Auk*, Vol. I, No. 1, pp. 305–310. Records by E. H. Eames of the capture of an adult male at Bridgeport, Conn., on May 16, 1889, and hearing this bird in full song. This is the seventh one known.

'Notes from Connecticut,' by E. H. Eames, Bridgeport, Conn., *Auk*, Vol. X., No. 1, pp. 89–90, 1893. Mr. Eames records: 'Four Lawrence's warblers were within a radius of half a mile, three typical and one with the black obscured and the crown dull yellow-olive, * * *.' This brings the known number of this bird up to eleven.

'Notes on *Helminthophila chrysoptera, pinus, leucobronchialis* and *lawrencei* in Connecticut,' by John H. Sage, Portland, Conn., *Auk*, Vol. X., No. 2, pp. 208–209, 1893. Mr. Sage records a single example, a male taken on May 14, 1887. This is the twelfth known specimen.

'Notes Concerning Certain Birds of Long Island, N. Y.,' by William C. Braislin, M.D., *Auk*, Vol. XX., No. 1, pp. 50–53, 1903. "At Cold Spring Harbor, Long Island, May 8, 1902, a specimen of Lawrence's warbler was secured. * * * is a male, and seems perfectly typical." This appears to be the thirteenth bird of this kind recorded.

Cf. Thurber, *True Democratic Banner* (newspaper), Morristown, N. J., November 10, 17 and 24, 1887. Records a specimen.

[15] '*Helminthophila leucobronchialis,*' by Robert Ridgway, *Auk*, Vol. II., No. 4, pp. 359–363. Ridgway argues from the color pattern of the type and the second recorded bird (see above), and

that were like *lawrencei,* while, on the other hand, we have three records of the breeding of *lawrencei.*[16] First, a female feeding young (*it is to be noted in this case both parents were H. lawrencei*), the male having been shot just previously. There were six young in this brood, which was not further disturbed, and several of the nestlings, presumably young males, showed traces of black on the throat. Second, a female *H. lawrencei* discovered attending a brood of what appeared to be young *H. pinus.* The male parent was not seen. Third, a male *H. lawrencei* mated with a female *H. pinus,* both parents attending six young (in the nest) which resembled in plumage typical nestlings of *H. pinus.*

Moreover, the number of known specimens (plus twenty) is in itself an argument against the theory of hybridity difficult to overcome. As before stated, I believe that here again we have a mutation from *H. pinus,* which has not flourished to the extent that has *H. leucobronchialis.*

The next fifty years should go far toward telling the story in regard to both of these birds and it behooves *every good field naturalist not to add more specimens of these birds to our collections, but to carefully observe them as they exist, alive;* to make, if possible, a comparative census of them in given localities where they are of regular occurrence, and to do this annually for many years to come. Much light, too, may be thrown on their relationship by observing with greater care than has heretofore been given the parentage of *all* the different nests of *Helminthophila,* in any

an adult female taken at Highland Falls, N. Y. (cf. Brewster), that these three birds are hybrids between *H. chrysoptera* and *H. pinus.*

'The Coloration and Relationship of Brewster's Warbler,' by Gerald H. Thayer, Monadnock, N. H. Regards *H. lawrencei* as a hybrid between *H. pinus* and *H. chrysoptera* because of its color pattern and its rarity.

Cf. Bishop, *Auk,* Vol. XX., No. 1, pp. 21–24, 1905.

[16] 'Connecticut Notes,' by A. H. Verrill, New Haven, Conn., *Auk,* Vol. X., No. 3, p. 305, 1893. Mr. Verrill writes: "On May 6, 1893, * * * I procured an adult male Lawrence's warbler. May 31, I noticed a Lawrence's warbler which I thought was breeding. On June 5 I again noticed the bird and shot it, and, after hunting some time, I finally flushed the female from her nest which, unfortunately, contained six young birds. I had a very good chance to examine her as she was constantly within six or eight feet from me. The nest was in all respects precisely like that of the blue-winged warbler. The young birds were well feathered out, and several of them showed traces of black on the throat."

["The really unfortunate part of the affair seems to have been not that the writer was disappointed in his hopes of a set of eggs, but that he failed to capture and rear the young and to secure the female—that he threw away a rare opportunity of casting much light on the status of this doubtful species.—Eds." *Auk.*]

'Connecticut Notes,' by Clark Greenwood Voorhees, New York City, *Auk,* Vol. XI., No. 3, pp. 259–260, 1894. "On the 12th of July (at Greenwich, Conn.), while looking for *Helminthophila,* I took an adult female, *H. lawrencei.* The bird is in every way like the female *H. pinus,* excepting that the thoat patch and stripe through the eye, which in the male *H. lawrencei* are black, are in this specimen dusky olive-green. The specimen is quite similar to the one taken by Mr. H. W. Flint in New Haven several years ago.

"The young in first plumage which this bird was attending when shot was in every respect typical *H. pinus.* The male parent was not found, but I feel confident that it was *H. pinus,* as the young were well feathered and showed clearly well-defined black lores of the latter."

'Breeding of Lawrence Warbler in New York City,' by C. William Beebe, curator of ornithology, New York Zoological Park, *Auk,* Vol. XXI., No. 3, pp. 387–388, 1904. Mr. Beebe records the discovery of this bird breeding in the Bronx Park. The birds were observed from May 18 until June 16, 1904. The nest was discovered early in June. A male *H. lawrencei,* typical in appearance, was mated with a female which appeared to be a typical *pinus.* On June 13 *both parents* were observed feeding the six young in the nest. The observers were within eight feet of the birds at this time. The nestlings upon examination were all in the *typical nesting plumage* of *H. pinus,* and showed no traces of the black markings of *H. lawrencei.* Very wisely these birds were not disturbed or collected and it will be interesting to watch future developments in this locality.

territory where *leucobronchialis* and *lawrencei* occur.

Thus far I have dealt with North American birds, but there is an additional instance from Italy that demands attention in this connection. In the year 1900 Professor Henry Hillyer Giglioli described a supposed new species of owl which he named *Athene chiaradiæ*.[17] This bird was discovered alive in the possession of a shoemaker at Caneva di Sacile. Its origin was traced back to a shepherd boy, who said that he took it from a nest in a crevice in a stone wall. There were four nestlings in this brood. After a day or two all but one of the young escaped. The locality Pizzocco is on the Prealps of Friuli.

This little owl was plainly related to a species, *Athene noctua,* common in this region, but it differed in having 'the tone and the pattern or style of the coloration,' so notable as to at once distinguish it from its ally; moreover, it had *dark brown irides,* which appeared black in the living bird. This in itself is remarkable, inasmuch as all the owls of the genus *Athene* have yellow irides.

By the year 1903 nine similar owls had been secured or observed, but all of them were found in nests, where some of their brothers or sisters were the yellow-eyed *A. noctua.* The parent birds of at least two of the nine known representatives of *A. chiaradiæ* are known to be true *A. noctua.*

These nine records were only secured after infinite painstaking effort, and I quote part of Professor Giglioli's conclusions in his article in the *Ibis:*

"And now for an attempt to explain the very strange and novel case. Of course, after what is now known, my first supposition that *A. chiaradiæ* might have been one of the last survivors of a species on the verge of extinction falls to the ground. But the opposite hypothesis, that we have in this singular small owl a case of *neogenesis* —*i. e.*, the *exabrupto* formation of a *new type* with sufficient differential characters to constitute, if maintained, a *new species*—can, I believe, be upheld.

"The term *neogenesis* was first used to explain this sudden origin of new forms from old-established species, if I am not mistaken, by my friend and colleague Professor Paolo Mantegazza, many years ago; it has been since used, more or less in the same sense, by the late Professor Cope and by others. I have no intention here of making any attempt to explain the causes which may bring forth such a result; they are necessarily various and usually occult. Suffice it to say that without a strong perturbation of the force of heredity such primary causes would give no result.

"Now, if in the case of *A. chiaradiæ* we have indeed an instance of true neogenesis—and the divergence of the parent birds from the normal type of *A. noctua* in different directions would go some way to prove that in them the force of heredity had been disturbed—we have before us an *attempt* at the formation of a new species, a case of singular and intense interest. I can not but consider it as an attempt, so far, for it is very possible that the couple of somewhat anomalous *A. noctua* now dead—which generated in all probability the *four* and perhaps *eight A. chiaradiæ* born at Pizzocco, and which possibly may also have been the parents of the couple from which the specimen at Fregona (at no great distance) was born—were *alone* endowed with the faculty of generating the black-eyed form, and they can do so no more. Again, should any of their black-eyed offspring have survived or should the occult primary causes leading to this singular case of neogenesis yet exist, and should in northeast Italy or elsewhere individuals of *A. chiaradiæ* be again produced and be able to breed freely, we can not guess whether or not the force of heredity, regaining its full sway, may fix, so to speak, the differential characters of specific value which suddenly emerged in the first specimens of *A. chiaradiæ,* or else, turning back to an easy atavism, alter the black-eyed form again to the original yellow-eyed *A. noctua.*

"In the first case a *well-defined* and *remarkable* species would be established; in the second my *A. chiaradiæ* would disappear. In either case I opine that the name that I have given to the black-eyed civetta should be maintained, for it is of obvious scientific interest to save this important case from oblivion. It will require several

[17] H. H. Giglioli, 'Intorno ad una presunta nuova specie di *Athene* trovata in Italia,' in *Avicula*, IV., fasc. 29–30, p. 57 (Siena, 1900). Reprinted in 'Ornis,' XI., p. 237 (Paris, 1901).

'The strange case' of *Athene chiaradiæ*,' by Henry Hillyer Giglioli, H.M.B.O.U., etc. *Ibid.*, Vol. III., 8th series, pp. 11–18, pl. I., 1903.

generations, under the most favorable hypothesis, viz., that more *A. chiaradiæ* be produced, to enable us to decide whether or not a new species of *Athene* has been formed.

"As to any other hypotheses to explain the formation of *A. chiaradiæ*, I can but repeat that I reject both that based on *hybridism*, and that of *a teratological* or *pathological* cause. Hybrids always show traces of the characters of both parents, especially when, as would be the case in *Athene*, of sheer necessity the connubium can not but occur with a species of such very distinct genera as *Nyctala*, *Scops* and possibly *Glaucidium;* now *A. chiaradiæ* is purely and simply an *Athene*, and shows no trace whatever of the characters, either specific or generic, of any of the forms quoted above. As to a teratological or pathological origin, a mere glance at one of the black-eyed civette will show that they can not owe their origin to such a cause. Besides in such cases, as again in hybrids, the form produced varies, and in these black-eyed descendants of *A. noctua* the specimens thus far examined are perfectly alike. The only instance in which we find perfect similarity in pathological descendants is in cases of absolute *albinism or melanism*, or, to put it better, in *monochroic* varieties.

"I believe that neogenesis gives a logical explanation of the strange case of *A. chiaradiæ*. But neogenesis, which appears to be of frequent occurrence amongst plants, has rarely been noted in animals, and I believe never before amongst vertebrata in a wild state.

"Finally, as I have said before, neogenesis may or may not lead to the establishment of a new species."

The conclusions arrived at by this eminent Italian naturalist, which have just been quoted at length, appeal to me strongly and force me to endorse the view he has so ably presented.

In the light of the evidence set forth only one answer can be made to the question as to the part that the process defined by de Vries as 'mutation' is playing among higher animals to-day. Beyond doubt we have witnessed the birth of new species of birds during the past seventy years. Moreover, some of these new species have flourished so as to have become a salient part of the bird fauna in the region where they occur and where they were unknown to skilled ornithologists, who carefully studied these regions in the early part of the last century.

William E. D. Scott.
Worthington Society for the Investigation of Bird Life.

BOTANICAL NOTES.

NORTH AMERICAN FLORA.

Some years ago a group of American botanists under the leadership of Professor Doctor N. L. Britton proposed to undertake the preparation of a comprehensive botanical work which was to bear the name 'Systematic Botany of North America.' One part, consisting of a few pages, was issued, since which nothing further has appeared. Botanists everywhere will be much pleased to know that in this interval work has gone forward, and that publication has been resumed. The title is now 'North American Flora' (instead of 'Systematic Botany of North America'), and its scope has been considerably extended, so as now to include the whole of North America from Greenland to Panama and the West Indian Islands.

As projected the work will include thirty volumes, which are to appear in from 120 to 150 'parts.' The volumes have been assigned as follows: Vol. 1, Mycetozoa, Schizophyta, Diatomaceae; 2 to 10, Fungi; 11 to 13, Algae; 14 and 15, Bryophyta; 16, Pteridophyta and Gymnosperms; 17 to 19, Monocotyledons; 20 to 30, Dicotyledons.

The magnitude of the work may be estimated from the fact that the part before us includes eighty pages. It will be published by the New York Botanical Garden, through the aid of a fund bequeathed by Charles P. Daly. The first part issued (bearing date of May 22, 1905) is Part 1 of Volume 22, beginning with the order Rosales, under which are monographed the families Podostemonaceae (by G. V. Nash), Crassulaceae (by N. L. Britton and J. N. Rose), Penthoraceae and Parnassiaceae (by P. A. Rydberg). The descriptions are concise and the synonymy full. Type localities, distribution and illustrations are cited. Metric measurements are used exclusively. Keys to families, genera and species are given.

The printed page is large (123 by 200 mm.), and the type and arrangement, while so compact as to leave no waste space, are pleasing to the eye.

NEW EDITION OF BRITTON'S MANUAL.

THE second edition of Britton's 'Manual of Flora of the Northern States and Canada,' which appeared some months ago includes descriptions of about one hundred additional species in an appendix, bringing the total number up to more than 4,600. Generic and specific synonyms have been added in many instances, thus adding greatly to the usefulness of the book for working botanists. The addition of a number of artificial keys will be especially helpful to beginners.

A NEW TROPICAL FLORA.

J. R. JOHNSTON (Gray Herbarium of Harvard University) has in preparation a work on the 'Flora of the Islands of Margarita and Coche' off the north coast of Venezuela, which must prove of much interest to American botanists. In noticing his descriptions of new species from these islands some time ago, the authorship of this work was erroneously given in these columns.

THE TEACHING OF BOTANY.

IN a most suggestive book entitled 'The Teaching of Biology in the Secondary School' (Longmans, Green & Co.) by Professors Lloyd and Bigelow, the former discusses many matters connected with the teaching of botany. Calling attention to the advances which botanical science has made in America during the last twenty-five years, and the changes which the teaching of the subject has experienced, he insists that the teachers should come to their work 'with a special mental equipment for their peculiar tasks,' and full of knowledge of the problems which they will be called upon to face in their work. In the course of the author's discussion one finds such chapter headings as 'The Value of Science, and Particularly of Biology in Education'; 'Nature Study; The Value of Botany in Secondary Education'; 'Principles Determining the Content of a Botanical Course'; 'The Various Types of Botanical Courses'; 'Use of the Method of Thought in Teaching Botany'; 'General Botanical Principles to be Emphasized in Teaching'; 'Detailed Discussion of the Course in Botany for the High School'; 'The Laboratory, its Equipment, Materials for Study and for Demonstration'; 'Botanical Literature for the Use of Teachers and Students.'

It is impossible to summarize these chapters. They should be read from beginning to end by every young teacher, and by some who are no longer young. In passing it may be noted that the author is thoroughly and heartily a believer in 'nature study'; indeed, he is so much in earnest in its advocacy that he devotes a good many pages to criticism of many of the erroneous methods employed by some of its teachers. In discussing the types of botanical courses for high schools he says truly, "one of the big ideas which a student should get from the study of plant forms is that of evolution. He should have an opportunity of looking into the kind of evidence which underlies this idea." The 'Huxley and Martin method,' he says, 'was ordinarily that of verification, while the development of individual initiative in thought was largely ignored.' Agassiz's method of bringing the student into 'direct contact with some form, such as a starfish, and leaving him to find out things for himself without aid of any kind' is characterized as 'heroic treatment, which can not be employed generally.'

Not to attempt too much is insisted upon, and also that the botany of the beginner must include something of each of the greater divisions of the science. In elucidating these suggestions the author discusses in detail the work which may be taken up in the high school. After quoting the course of study recommended by the Committee on a College Entrance Option in Botany, of the Society of Plant Morphology and Physiology, he details a course of his own, beginning with morphology and anatomy of the fruit and seed, and following this with ecology, field work, physiology, the root, the shoot, the leaf, the bud, Myxomycetes, Schizophyta, Thallophyta, Bryophyta, Pteridophyta, Phanerogams, geographical botany and physiographical plant ecology. In practise it will be found quite

impossible to cover this work in the time allotted to botany in the secondary schools, but tnere can be no doubt as to the high value of these suggestions, from which the teacher may well make such selections as his time may permit.

Charles E. Bessey.
The University of Nebraska.

CURRENT NOTES ON METEOROLOGY.

FOEHN WINDS IN THE ANTARCTIC.

During the Antarctic voyage of the *Discovery*, warm southerly winds were observed which, because of their high temperature, have generally been regarded as of *foehn*-like character. Sir Clements Markham (*Geogr. Journ.*, June) believes that the high temperature may result from the fact that these winds blow from the ocean beyond the pole, that is, Weddell Sea, and not from adiabatic warming during descent. Hence he thinks that the Great Barrier may end on the other side of the pole with another line of ice-cliffs facing the Weddell Sea, and that the winds may blow across the ice barrier with great velocity without lowering their temperature. On the other hand, Dr. W. N. Shaw suggests that the snow which comes with these warm southerly winds is carried along in a surface drift, and notes that intensely cold air can contain very little moisture.

LOW TEMPERATURE IN THE SAHARA.

In the *Meteorologische Zeitschrift* for June, 1905, there is a note on some low temperatures observed on December 19, 1904, in the Sahara, between Tuggurt and Guerrara. The temperature at midnight was 30.2° Fahr.; at daybreak (6:15 A.M.), 28.4°; at sunrise (7:15 A.M.), 33.8°; at 2:30 P.M., in the shade, 75.2°; at 7 P.M., 41.0°; and at 8:30 P.M., 39.2°. It was calm, and the sky was clear. On December 20, at 7:30 P.M., the temperature was 33.8°, and there was heavy frost, which in places reached a thickness of 1 cm.

NOTES.

Das Wetter for June, 1905, contains an interesting article, of a 'popular' nature, entitled 'Aus dem Leben der Wolken,' by Dr. A. de Quervain; also a discussion, illustrated by means of curves, entitled 'Temperaturen auf Bergstationen und in der freien Atmosphäre, by Dr. W. Wundt.

The *Annuaire météorologique* of the Royal Observatory of Belgium contains a useful list of text-books of meteorology, prepared by J. Vincent. Special attention is paid to *general* treatises, but a considerable number of special works on marine, medical and agricultural climatology are included. The list begins with Aristotle, and includes books in Latin, Greek, English, French, German, Italian, Dutch, Russian, Danish, Spanish, Hungarian, Norwegian and Portuguese.

The mechanism of the origin of rain-clouds, and the conditions of heavy rains and floods on the northern slope of the Pyrenees, were discussed by Marchand, Director of the Pic du Midi Observatory, before the Congrès du Sud-Ouest Navigable, held at Bordeaux in June, 1902. The paper was printed in the proceedings of that congress, and a German translation of a portion of the article, in the *Meteorologische Zeitschrift* for June, 1905, makes this interesting study accessible to the general reader.

Recent publications on the meteorology of the free air are those of Teisserenc de Bort, on the diurnal changes in temperature (*Comptes rendus*, Vol. cxl., 1905, 467) and of Hergesell, on the results obtained by means of kites over the Mediterranean Sea and Atlantic Ocean in 1904 (*ibid.*, January 30, 1905).

R. DeC. Ward.

ANNUAL REPORT OF THE COUNCIL OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The annual report of the council for the year 1904–5 states that the arrangements for the meeting of the association in South Africa had been directed, under the sanction of the council, by a special South African committee, sitting in London, and consisting of the general officers of the association (the president and president-elect, the general treasurer and the general secretaries), Professor Armstrong, Dr. Horace Brown, Sir William Crookes, Sir

James Dewar, Sir Archibald Geikie, Professor H. A. Miers, Sir Henry Roscoe and Dr. Sclater. The coordination of the work of the various local committees had been carried out under the direction of the central organizing committee for South Africa, sitting at Cape Town, consisting of Sir David Gill (chairman) and Dr. J. D. F. Gilchrist (secretary). An additional expenses fund having been opened to supplement the subvention of £6,000 from the South African colonies, contributions amounting to £3,100 had been received.

The following agreement has been made between the British Association and the South African Association in the matter of financial arrangements respecting the annual meeting in 1905: (1) That all members (but not associates) of the South African Association shall be entitled to associates' tickets at the meeting of the British Association in South Africa in 1905; (2) that the South African Association shall pay a contribution of £500 to the funds of the British Association; (3) the South African Association guarantees the purchase of a thousand copies at least of the annual volume, the copies to be sent direct to the members of the South African Association on payment to the British Association by the South African Association of the sum of 8s. per copy.

A committee of the council, consisting of Professor G. H. Darwin, Sir A. Geikie, the general secretaries and the general treasurer, was authorized to consider the appointment of an assistant secretary, in succession to Dr. Garson, resigned, with the result that Mr. A. Silva White was unanimously appointed to fill that office.

The books and other publications presented to or received in exchange by the association, with the exception of the publications of the corresponding societies of the association and the annual volumes of reports of the various Associations for the Advancement of Science, have been transferred to the Library of University College, Gower Street, the council of University College having undertaken to give the same facilities to members of the British Association for the use of University College Library as were granted under similar circumstances by the University of London.

The council also reported on a plan for dealing with the meteorology of the British colonies and the relation of the association to corresponding societies.

SCIENTIFIC NOTES AND NEWS.

THE American Anthropological Association is meeting this week at San Francisco under the presidency of Dr. Frederic Ward Putnam, of Harvard University and the University of California. The preliminary program contains the titles of thirty-nine papers, which proves that the anthropologists at least can hold an unusually successful meeting in the summer and on the Pacific Coast. We hope to print subsequently abstracts of the papers.

MR. W. R. DUNSTAN, F.R.S., director of the scientific and technical department of the Imperial Institute; Mr. F. W. Dyson, F.R.S., chief assistant at the Royal Observatory, Greenwich, and Dr. R. T. Glazebrook, F.R.S., director of the National Physical Laboratory, have been elected members of the council of the British Association.

THE regents of the University of California have granted a year's leave of absence to Professor Wm. E. Ritter, of the department of zoology, for research at the San Diego Marine Biological Station and travel abroad. Associate Professor Charles A. Kofoid will have charge of the department in his absence. Mr. C. O. Esterly, Mr. L. Griggs and Dr. Alice Robertson have been appointed assistants in zoology.

MR. GEORGE K. CHERRIE, of the Museum of the Brooklyn Institute, has just returned from South America, where he has been collecting for that institution. He obtained about 800 bird skins representing very fairly the avifauna of the region about Ciudad Bolivar, Venezuela. These include a fine series of the Hoatzin, together with nests and eggs of that bird; skins and skeletons of the Guacharo bird, and skins of a number of species of South American herons. From the observations of Mr. Cherrie, it seems probable that the breeding of the hoatzin is largely influenced

by the condition of the water, and inland, away from the influence of tide water, they do not breed until the ground beneath their nests is flooded. For this reason, although Mr. Cherrie stayed until June, he only obtained eggs of this bird. Eggs of a number of other species of birds were also obtained, many of which are but little known.

Mr. Edward W. Berry, the paleobotanist and secretary of the Torrey Botanical Club, is engaged in studying the fossil flora of Maryland for the Geological Survey of that state. Correspondents are requested to address him after September 1, in care of the Maryland Geological Survey, Johns Hopkins University, Baltimore, Md.

Mr. Gerald Dudgeon has been appointed by the secretary of state for the British colonies to examine and report upon questions relating to the development of the agricultural resources of British West Africa.

Dr. Oliver E. Glenn, acting professor of mathematics at Drury College, has been appointed a member of the editorial staff of the *American Mathematical Monthly*, succeeding Dr. Saul Epsteen, who has been called to the University of Colorado.

Professor J. Volhard, professor of chemistry at Halle, celebrated on August 6 the fiftieth anniversary of his doctorate.

The trustees of the British National Portrait Gallery have purchased a portrait of Tiberius Cavallo, 1746–1809, one of the earliest students of electrical science.

We learn from the New York *Evening Post* that the city of Nuremberg, in conjunction with the Society of German Clockmakers, has erected a monument by way of commemorating Peter Henlein, who, four hundred years ago, substituted springs for weights in clocks, and thus made watches a possibility. The statue was made by the Berlin sculptor Meissner. It represents Henlein at work in his shop.

In connection with the indication by the council of houses in London which have been the residences of distinguished individuals, a memorial tablet was on August 14 erected on No. 34, Gloucester Square, Hyde Park, where Robert Stephenson, the great engineer, resided at one time. The tablet is of encaustic ware and terra-cotta in color.

Dr. Otto Herz, the entomologist of St. Petersburg, has died at the age of fifty-six years. The deaths are announced of the Rev. Dr. J. Keith, of Scotland, who took an active interest in natural history, and of Mr. W. E. Langdon, a past president of the British Institution of Electrical Engineers.

We recorded last week the death of Professor Leo Errera. A correspondent writes in regard to him: "Errera was one of the comparatively few rich men who work as hard at science as do those who earn their living by teaching and research. A multi-millionaire, he earned the rather hard degree of Docteur Agrégé, and since his student days has been an important factor in the class-room and research work in botany at Brussels. His most notable work as an investigator, perhaps, was his demonstration that fungi store food reserves in the form of glycogen, like animals, and the contributions from his laboratory have been chiefly along physiological and ecological lines. Notwithstanding this limit to the field of his more active contributions, however, he was interested in all branches of botany, and at the recent International Congress at Vienna, where he secured a decision to hold the next quinquennial session at Brussels, he was one of the most constant and interested attendants at the arduous—and perhaps thankless—nomenclature sessions.

The correspondent of the London *Times* in Cape Town cabled on August 18 that the first boat, conveying a portion of the visiting members of the British Association to Durban, and marking the conclusion of the business program in Cape Town, would leave that night. It was obvious at the outset that a rivalry was established between the purely pedagogic portion of the program and that part which makes for first-hand knowledge of South Africa. Fortunately there is no tendency to gauge the success of the 1905 meeting by the attendance at the lectures. So far the attendance has not been striking, but it is felt that

that has been more than made up by the keenness of the members to see all that can be seen of the country, and to profit by a closer acquaintance with its problems, which is essential to the proper understanding of them in England. South Africa, while paying a tribute to the high standard of the papers read, will endorse whole-heartedly the policy adopted by the members.

THE Committee of the British Association on Zoology Organization has reported that a register of zoologists has been established, and that fifty-seven zoologists have accepted the invitation of the committee to place their names upon the register. The committee has obtained by correspondence the opinion of a large number of the zoologists of the country upon the question of the importance of the grant applied for by the committee of Section D to enable a committee to send a competent investigator to the Zoological Station in Naples. Other matters affecting the interests of zoologists in the country have engaged the attention of the committee during the year. A meeting of the committee was held in London on May 11. A meeting of zoologists summoned by the committee to consider the question of the teaching of natural history in schools was held in the Zoological Gardens, London, on the same date.

AT the last monthly meeting of the Zoological Society of London it was stated that the additions to the society's menagerie during that month had amounted to 274, amongst which special attention was called to a leopard (*Felix pardus*), from near Hong-kong, presented by Mr. J. A. Bullin; to the three Californian sea lions (*Otaria gillespii*), from Santa Barbara, purchased; to a white-tailed gnu (*Connochætes gnu*), born in the menagerie; and to a male Somali ostrich (*Struthio molybdophanes*), purchased.

IT is expected that within a year wireless telegraph communication will be established between New Zealand and Australia.

THE *Journal of the American Medical Association* states that, at the request of the medical directors of the City Hospital, the board of public service adopted a resolution that hereafter the professor of pathology of the Cincinnati University shall be the head of the pathologic department of the hospital. The directors stated that they had been made convinced of the fact that a professorship of pathology would soon be added to the university staff.

THE Sanitary Inspectors' Association met in London on August 18. In the course of his presidential address, as reported in the London *Times*, Sir J. Crichton-Browne dealt with the housing problem, and pointed out the advantages from a health point of view of country life as compared with town life. That the townsman was shorter-lived than the countryman was, he said, incontrovertible. Professor Karl Pearson, a thoughtful and cautious anthropologist, had told us that decadence of character and of intelligent leadership was to be noted alike in the British merchant, the professional man and the workman, and this he attributed to the fact that the intellectual classes were not reproducing their numbers as they did 50 or 100 years ago. In this view Professor Pearson was supported by the prime minister, who said at Cambridge last year that in the case of every man who left the laboring class and became a member of the middle or wealthier classes his progeny were likely to be diminished owing to the fact that marriages were later in that class. He was inclined to think, however, that intellectual decadence, if it be upon us, was not altogether due to the causes assigned by Professor Pearson and Mr. Balfour, and was not necessarily destined to deepen as time went on. In a people like our own there was always outside the actually intellectual class a still larger class potentially intellectual with abilities incompletely evolved, because never called forth, but capable under stress of circumstances of the higher development. Many of our finest intellects had sprung from the unintellectual class, and genius was generally more or less of a sport. His own view was that any dearth of ability from which we might be suffering was to be ascribed not so much to the infertility of the cultivated classes as to the artificial production of stupidity in various ways, and to the incessant drainage from the country—which was the fit and

proper breeding place and rearing ground of intellect—of the best elements of our people to be swallowed up or deteriorated in our big towns. Not less untenable than the notion that the agricultural laborer was dull of intellect was the idea that the city urchin was cleverer and better endowed mentally than the little yokel. The rule seemed to be that the mental development of children was hastened by city life, but soon stopped short. Up till thirteen or fourteen years of age they were precocious and then came to a standstill. City life at its best was bad for children, involving, as it did, early puberty, exciting distraction, superficiality of knowledge, insufficient repose and the want of soothing influences that the country afforded; and at its worst, when it meant a tight squeeze in squalid dwellings, poor food, foul air, contact with vice and manifold temptations, it was utterly demoralizing. It seemed obvious that, if the city went on growing at the nineteenth century rate, and under nineteenth century conditions, it would dry up the reservoirs of strength in the population and leave an immense proletariat of inferior quality and without commanders.

UNIVERSITY AND EDUCATIONAL NEWS.

Moses A. Dropsie, of Philadelphia, formerly president of Gratz College, has bequeathed about $600,000, for the endowment of a college for the study of Hebrew literature and Rabbinical learning in that city. There are to be no restrictions in admission as to creed, sex or color.

The Department of Chemistry of Washington and Lee University is being reequipped this summer in new quarters. In all fourteen rooms will be occupied, nearly the whole of the old main building of the university being given over to the departments of chemistry, geology and biology.

Logan Hall, containing the anatomical and surgical departments of the University of Pennsylvania, was burned on August 14, entailing a loss of about forty thousand dollars. Many microscopes were destroyed.

We learn from *Nature* that probate has been granted of the will of Mr. John Innes, of Merton, Surrey, who died on August 8, 1904, leaving the sum of about £200,000 for public and charitable purposes. Among other bequests he left his house, the Manor Farm, Merton, and two acres of ground, "to establish thereon a school of horticulture or such other technical or industrial institution as the law will allow, to give technical instruction in the principles of the science and art of horticulture and the necessary physical and mental training incidental thereto; to erect suitable buildings and furnish them, and to provide workshops, tools, plant, scientific apparatus, libraries, reading-rooms, lecture and drill halls, a swimming bath and gymnasium. If this may not be legally carried out, then to establish in these buildings a public museum for the exhibition of collections of paintings and similar works of art, objects of natural history, or of mechanical or philosophic inventions, and to lay out land for a park."

The Draper's Company has made a further grant of £5,000 for an extension of the premises of the East London Technical College.

The University of Melbourne has received a largely increased endowment from the government of Victoria on condition of instituting a course for a degree in agriculture. The necessary arrangements for such a course have now been made, and the university is inviting applications in England and America for a professorship of botany and a lectureship in biochemistry in connection with the school of agriculture. A new professor of anatomy is also to be appointed for the medical school.

Mr. S. Herbert Cox has been appointed to the professorship of mining at the Royal School of Mines, South Kensington, vacant on the death of Sir Clement le Neve Foster.

Mr. Edward Ward, who has held the professorship of surgery in Leeds for the past six years, has resigned and will be succeeded by Mr. Harry Littlewood.

Dr. Richard Willstäter, associate professor of chemistry at Munich, has resigned.